Non-Standard Preferences and Beliefs in Financial Decision Making

D I S S E R T A T I O N

zur Erlangung des akademischen Grades
doctor rerum politicarum
(Doktor der Wirtschaftswissenschaft)

eingereicht an der

Wirtschaftswissenschaftlichen Fakultät
der Humboldt-Universität zu Berlin

von

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Tag des Kolloquiums: 25.März 2020

Dedicated to my father
Werner Koch

Acknowledgements

I gratefully acknowledge that, during my doctorate, I was a Ph.D. student of the Graduate Center of the German Institute for Economic Research (DIW Berlin) and a research associate in the International Economics department at the DIW. In addition, I was financially supported by the Heinrich Böll Foundation.

I would like to thank my first supervisor and co-author Lukas Menkhoff, who supported and encouraged me throughout my doctorate with his words of advice. Thank you for providing me with such a great research environment and for showing me how to be an honest and prudent researcher. I am deeply indebted to you for giving me the opportunity to do research in a country that I feel so attached to. I also want to thank my second supervisor Georg Weizsäcker for showing me the world of behavioral economics in my master studies when I was almost about to quit and his support throughout my studies.

I am grateful to my co-authors Michelle Brock, Antonia Grohmann, Theres Klühs, Ulrich Schmidt, and Wiebke Stein. I often rely on your advice and learned a lot from you. Thank you for listening to my ideas and being so dedicated to our projects. I also want to thank Adam Lederer for proof-reading this dissertation.

Furthermore, I want to thank my colleagues in the International Economics department and my fellow GC-students, especially Patrick Burauel, Daniel Gräber, Jana Hamdan, Sven Hartjenstein, Jakob Miethe, Renke Schmacker, Helke Seitz, Kevin Tran, and Aline Zucco. My research profited a lot from our discussions over countless coffees. A special thanks goes to the whole GC-2015 cohort without whom the doctorate would have never been so much fun and enlightening.

Eventually, I want to thank my family and friends. You always cheer me up when times are rough. Fabian, thank you for always being there as a partner, friend, and colleague. Most of all, I thank my parents for their unconditional love and support. The chances for a person with my socio-economic background to obtain a PhD are almost zero. You take all the credit for rigging this lottery and turning the odds in favor for me. This dissertation is dedicated to my father who does not live to see me graduating.

Summary

Financial resilience and managing financial risks are key factors of a successful financial inclusion. The personal factors that shape financial management are, yet, not well understood. This dissertation studies how non-standard economic preferences and beliefs might help explain different financial management practices of households. The focus is on countries that are on the verge of becoming high income economies and where financial products and inclusion are steadily expanding. Four domains of financial risk management are considered. Chapter two analyzes the relationship between inequality aversion and insurance take-up. To this end, a novel measure for inequality aversion is constructed and employed in a household panel survey in Thailand. In chapter three, the effect of social comparison on debt taking is investigated in a lab experiment in Germany to disentangle two kinds of peer effects: social image concerns and peer information. Chapter four explores potential differences in uncertainty preferences and in beliefs between supposedly high-risk managers: necessity and opportunity entrepreneurs as well as return migrants in Albania and Kosovo. In chapter five, beliefs and their potential effect on over-indebtedness are studied using the same panel sample in Thailand as in chapter two. All chapters follow a common methodological approach by using lab(-in-the-field) experiments. In three chapters, lab evidence is set in relation to real life outcomes elicited with self-reported survey data.

Deutsche Zusammenfassung

Finanzielle Resilienz und der Umgang mit finanziellen Risiken sind wesentliche Bestandteile einer erfolgreichen finanziellen Inklusion. Die persönlichen Faktoren, die finanzielles Management formen, sind allerdings nicht umfassend bekannt. Diese Dissertation untersucht wie nicht-standard-ökonomische Präferenzen und Vorstellungen („Beliefs“) dazu beitragen können verschiedene Vorgehensweisen im finanziellen Risikomanagement von Haushalten zu verstehen. Der Fokus liegt dabei auf Ländern, die kurz davor stehen einkommensstarke Ökonomien zu werden und in denen die Auswahl an finanziellen Produkten und die finanzielle Inklusion stetig wachsen. Vier Bereiche des finanziellen Risikomanagements werden betrachtet. Kapitel zwei analysiert den Zusammenhang zwischen Ungleichheitsaversion und der Aufnahme von Versicherungen. Dazu wird ein neuartiges Maß für Ungleichheitsaversion konstruiert und in einer Haushalts-Panelumfrage in Thailand verwendet. In Kapitel drei wird der Effekt von sozialen Vergleichen auf die Schuldenaufnahme in einem Laborexperiment in Deutschland untersucht um zwei Arten von Peer Effekten zu entflechten: Sorge um das soziale Ansehen und Peer Information. Kapitel vier erforscht potenzielle Unterschiede in Unsicherheitspräferenzen und in Beliefs zwischen Individuen, die vermeintlich ein hohes Risiko managen: Selbständige aus der Notwendigkeit und Selbstständige aus der Möglichkeit heraus als auch Menschen mit Migrationsgeschichte in Albanien und im Kosovo. In Kapitel fünf werden Beliefs und deren potenzieller Effekt auf Überschuldung innerhalb der gleichen Panel-Stichprobe in Thailand wie in Kapitel zwei studiert. Alle Kapitel folgen einem gemeinsamen methodologischen Ansatz indem Labor- oder sogenannte lab-in-the-field-Experimente verwendet werden. In drei Kapiteln wird die Evidenz aus dem Labor in Relation zu Resultaten aus dem wahren Leben gesetzt, die mit selbstberichteten Umfragedaten erfasst werden.

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General Introduction

> Lesson no. 1: Making comparisons can spoil your happiness. [...]
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>
> *Hector and the Search for Happiness*
> *François Lelord (2010)*

Managing financial risks and being financially resilient are key elements of successful financial inclusion (Karlan and Morduch, 2010). Not just unexpected shocks can have severe consequences for individuals, but also expected expenses, if they do not have the means to cover these. Thus, deciding which risks to insure and which risks to take in the first place are crucial components of household financial management. For example, borrowing enables individuals to invest in assets they could otherwise not afford but bears the risk of defaulting if they are not resilient to financial shocks or if they mismanage their finances. In addition, especially in settings where risk is frequently beyond one's control, building resilience is an important policy goal of development banks around the world (Asian Development Bank, 2019; Demirgüç-Kunt et al., 2018).

Financial risk management can build on insurances and safety nets, on savings or assets, on borrowing, even on changing occupations, or on changing liability structures. Risk management not only means being prepared for shocks but also making it possible to take risks that potentially entail larger benefits. As positive correlations between risk and expected gains can be found in various domains, having no insurance and being fully liable, just like lacking savings and opportunities to borrow, can prevent people from more profitable investments. For example, having no insurance might hamper investment in more risky but, in return, more profitable crops (e.g. Cole et al., 2017; Karlan et al., 2014). Similarly,

being self-employed might lead to higher income or independence but due to high individual liability, some are deterred from entering the market (e.g. Hvide and Moen, 2010; Paulson and Townsend, 2004).

A central question that emerges is what eventually determines how people manage financial risks and how they decide which risks to take. Naturally, there are binding factors that are not in the hand of the individual, e.g. access to finance and insurance, as well as legal liability structures, that limit or change financial risk management (e.g. Beck et al., 2008; Bruhn and Love, 2014; Burgess et al., 2005). Beyond this, household characteristics are still an important determinant of financial management, as even at high levels of financial inclusion there is substantial heterogeneity across household management practices. There are "hard," socio-economic factors, like initial wealth, income, age, and sex of the household head, that partially explain the prevalence of borrowing, entrepreneurship, insurance, and savings (see Beshears et al., 2018). Yet, there are still many unresolved questions, especially with regard to household debt management (Zinman, 2015), insurance take-up (Baicker et al., 2012; Cole et al., 2013), and decisions to become entrepreneurs (Astebro et al., 2014).

Standard theoretical models fail to explain the high prevalence of household debt and entrepreneurial activity as well as the lack of insurance. Non-standard preferences and beliefs, not captured in these models, seem to be a fruitful avenue in trying to better understand household risk management. Non-standard uncertainty preferences, like loss aversion or ambiguity aversion, are the most obvious starting point. There is an increasing, albeit small, body of literature on the relation of these to real-life risk management. Other non-standard, for instance social, preferences seem to play an important role in managing financial risk as well (see Beshears et al., 2018, for an overview). Furthermore, individual beliefs are crucial when outcomes are risky or uncertain (Fox and Tversky, 1998). However, the impact of these on real-life financial decisions is still not well researched.

These considerations are also true for emerging markets and middle income countries, where a tremendous increase in financial inclusion and opportunities and, at the same time, still fragile political and economic systems, ask for explicit research on households' financial risk management (IMF, 2017). For example, in countries like Albania or Thailand, which are both upper-middle-income countries, most people still report relying on money from family and friends if an emergency occurs, which is an incomplete form of insurance (Karlan and Morduch, 2010). In other aspects, the two countries are completely different: in

Thailand the share of households with savings is extremely high in comparison to Albania; while, simultaneously, the household debt to GDP ratio is eight times larger in Thailand (IMF, 2017; World Bank, 2017). More research is needed on how preferences and beliefs potentially shape these outcomes.

This dissertation concentrates on the role of non-standard preferences and beliefs in managing financial risks. Thereby, a special focus is set on upper-middle-income countries. The dissertation contributes to understanding how behavioral factors, in comparison to socio-demographic factors, affect financial decisions and risk management. The four chapters address four areas of financial risk management: insurance take-up, borrowing, entrepreneurship, and migration. Chapters two and three concentrate on non-standard preferences, chapter five rather focusses on beliefs, while chapter four explores both.

Chapter two analyzes the relationship between inequality aversion and insurance take-up. To this end, a novel measure for inequality aversion is constructed and employed in a household panel survey in Thailand. In chapter three, the effect of social comparison on debt taking is investigated in a lab experiment in Germany to disentangle two kinds of peer effects: social image concerns and peer information. Chapter four explores differences in uncertainty preferences and beliefs between supposedly high-risk managers: necessity and opportunity entrepreneurs as well as return migrants in Albania and Kosovo. In chapter five, beliefs and their potential effect on over-indebtedness are studied using the same panel sample in Thailand as in chapter two. All chapters follow a common methodological approach by using lab(-in-the-field) experiments. In three chapters, lab evidence is set in relation to real life outcomes elicited with self-reported survey data.

Summary of the Dissertation

In chapter two, which is joint work with Lukas Menkhoff and Ulrich Schmidt, a new measure for inequality aversion is developed. The measure is called *coupled lotteries* because two persons face identical binary lotteries and the only decision they have to take is if they want to play the lotteries coupled or separated. Coupled means the lotteries are perfectly positively correlated and separated means the lotteries are drawn independently from each other. If lotteries are coupled, there is no outcome inequality. Hence, we obtain an easy, cheap, fast, and ready-to-use measure for inequality aversion, which is an advancement in the literature. Previous measures are either not able to clearly disentangle inequality

aversion and other social preferences or are more complicated to assess, requiring various structural assumptions.

Our method is tested in a panel survey in rural Thailand and supplementally in a lab in Germany. First, we confirm the existing literature by finding that our measure for inequality aversion is related to other individual preferences, namely risk aversion and social status concerns. Second, as most of the rural, agricultural households in Thailand are still not sufficiently insured, we analyze if those household heads who are inequality averse are more likely to have some kind of formal or informal insurance for their households. Theoretical considerations hypothesize a positive relationship between inequality aversion and insurance, which is confirmed by our analysis. Finally, we do not find that coupling is correlated with giving in the dictator game, which is one of the commonly used, albeit flawed, measures for inequality aversion.

Thus, we contribute to the literature by constructing a more rigorous measure for inequality aversion and by providing empirical evidence for a link between inequality aversion and the inclination to insure. So far, there is only little evidence on how inequality aversion might be related to managing financial risk. Our study helps shed light on one particular aspect.

Chapter three, co-authored with Antonia Grohmann, contributes to the relatively small literature on determinants of household debt. Using a lab experiment in Germany, we try to disentangle two channels through which social comparison -comparing with others- might influence consumption and subsequently debt taking: we isolate social image concerns and peer information effects. Additionally, we study if certain character types are more prone to social comparison effects.

There are still not many studies investigating if people are willing to finance consumption out of social comparison motives with taking a credit. However, borrowing to finance conspicuous consumption could contribute to the steep increase in household debt seen around the world. Furthermore, in comparison to social image concerns, peer information effects are even less frequently analyzed in the nexus of consumption and borrowing (see Bursztyn and Jensen, 2017).

We find that in our setting, social image concerns do not lead to more debt taking but peer information does. Social image concerns actually lead to less debt taking as participants have social image concerns of a different kind than we expected. Surprisingly, we find a rather large effect of peer information that is likely to be driven by a preference for conformity. Participants moderately converge to the choices of others, which results in less or more debt taking depending on where they are placed in the distribution of incomes.

We lack power to precisely measures effects for most of the character traits but we believe this is a promising direction for future research as we find some evidence for heterogenous effects. Most of all, social comparison effects in financial decision making seem to be important determinants that are understudied so far.

Chapter four, co-authored with J. Michelle Brock, analyzes potential differences in ambiguity aversion attitudes and uncertainty preferences between entrepreneurs out of necessity and those out of opportunity as well as compares both to another group of occupational risk-takers, return migrants. Furthermore, before eliciting preferences for strategic and non-strategic uncertainty, we apply a treatment. The treatment is supposed to exogenously vary the perceived level of competence with respect to judging other people's market entry decision.

The difference in willingness to enter the market and the frequently observed difference in profitibality between necessity and opportunity entrepreneurs' businesses is still not well understood. Our study concentrates on uncertainty preferences and beliefs, which are understudied in the general literature on entrepreneurship. The literature on whether entrepreneurs are more risk taking than others is inconclusive, which could be (i) because entrepreneurs do not differ in their willingness to take risks but in their willingness to take uncertainty and their perception of uncertainty; and (ii) because another group of occupational risk-takers in the comparison group confounds the results: migrants. Therefore, we concentrate on non-standard uncertainty preferences and isolate return migrants to compare them separately to entrepreneurs.

Although we do not find very pronounced differences in ambiguity aversion across the groups, opportunity entrepreneurs and returnees are significantly more a-insensitive than necessity entrepreneurs, which means they more often treat uncertain gambles like 50-50 chances. Furthermore, necessity entrepreneurs are less willing to take non-strategic uncertain gambles than the other two groups in our control treatment. However, they react strongest to our treatment and experience a large boost for tolerating uncertainty. Return migrants are, in turn, almost immune to the treatment. In general, it seems that the treatment does not increase perceived capability but rather the level of general optimism. Those who have the feeling they have no other choice than starting a business profit the most from this treatment, which is informative for business trainings and other policy interventions that seek to spur entrepreneurship and business survival.

In chapter five, after looking at how non-standard preferences affect debt taking, the dissertation turns to beliefs as another potential determinant in a setting where expectation formation is crucial. Observing the same households in

rural Thailand as in chapter two, Theres Klühs, Wiebke Stein, and I investigate if positive expectations about future household income increases the likelihood and severity of over-indebtedness. As previously noted, household debt in Thailand is extremely high and simultaneously, there is large income uncertainty, especially in rural areas where households mostly engage in agriculture. A downside of high financial inclusion in some emerging markets could be that insufficient screening actually harms households in the long-run as they borrow excessively.

We construct a survey specifically to collect extensive data on objective and subjective over-indebtedness indicators as well as to elicit quantitative income expectations. We use these measures for our estimations, where, importantly, we control for household characteristics and other factors like unexpected shocks that potentially confound the relationship between expectations and over-indebtedness. To identify one possible channel how expectations can lead to more debt taking, we also conduct a lab-in-the field experiment with the same households. In the experiment, we investigate the causal effect of over-confidence on borrowing.

Our results suggest a robust and strong link between positive income expectations and over-indebtedness. Results are slightly different for objective and subjective over-indebtedness indicators, but all point in the direction that positive expectations contribute to being over-indebted. This is supported by the additional result that households that are more certain about their expected income development are also more likely to be over-indebted. Although it might be rational to borrow more today if income expectations are justifiably higher, expecting too much and being too certain in an uncertain situation, such as the situation that rural, agricultural households in Thailand live in, seem to lead to serious financial distress. The survey results are supported by the lab results, which find overconfidence to be related to overspending in the game. Additionally, those households spending too much in the lab are also more likely to be over-indebted in real life. Still, mostly because of sticky and over-confident beliefs at baseline, we are not able to detect a causal link between overconfidence and overspending in the lab.

Chapter five is one of only a few studies empirically exploring the relationship between income expectations and household debt. Globally, there is still not much evidence and we are the first to quantify real-life income expectations and their potential effect on over-indebtedness.

Overall, this dissertation helps deepen the understanding of how non-standard preferences and beliefs shape financial risk management of households in diverse

settings. Since household debt is rising worldwide and over-indebtedness can put households in extreme distress and misery, it is crucial to understand the drivers of over-indebtedness and if there is potential to change them. Two chapters focus on sub-optimal borrowing from a policy perspective. There might be scope to refine household expectations and perceptions by providing sufficient information and training on information processing. In many cases, however, this is not feasible and equipping households with insurance and adequate safety nets still seems to be more appropriate. As seen in chapter two, the willingness to insure inherently depends itself on social preferences. Thus, it might be helpful to discuss new insurance products e.g. at village meetings. However, for other financial decisions, like borrowing itself, the influence of others might have adverse effects. A preference for conformity can lead to excessive debt taking and, possibly, the same may hold for insurance take-up. More research is needed to understand under which conditions social preferences lead to "better or worse" financial decisions. The same is true for beliefs. As shown in chapter five, being too optimistic potentially leads to financial distress, whereas in chapter four optimism is beneficial for those who are the most afraid of uncertainty. In that sense, the lessons described in the beginning seem not only to be lessons on happiness but also lessons on finances.

Chapter 2

Coupled Lotteries - A New Method to Analyze Inequality Aversion

We thank Giuseppe Attanasi, Colin F. Camerer, Dirk Engelmann, Menusch Khadjavi, Stefan Trautmann, Roel van Veldhuizen, Pauline Vorjohann, and seminar participants in Berlin, Hannover and Ohlstadt for helpful comments that markedly improved this paper. The paper also profited from discussions with conference participants at SABE/IAREP 2018 (London), EEA 2018 (Cologne), the VfS Annual Conference 2018 (Freiburg), NCBEE 2018 (Odense), ASSA 2019 (Atlanta) and RES 2019 (Warwick). We are grateful to Thore Petersen, who provided research assistance. Financial support by the German Research Foundation (DFG) via CRC TRR 190 is gratefully acknowledged.

2.1 Introduction

The pure self-interest hypothesis of standard economic theory is refuted by an overwhelming body of evidence from economics and psychology. The respective literature documents that people typically also have other-regarding preferences that include concerns for the resources of others as well as for reciprocity and fairness. One of the most prominent concepts in the literature on other-regarding preferences is inequality aversion. According to the seminal models of Fehr and Schmidt (1999) and Bolton and Ockenfels (2000), the utility of an inequality averse agent is increasing in both the own payoff and the equality of the distribution. The prominence of inequality aversion lies in the fact that it can explain observed behavior, which deviates from the self-interest hypothesis in many experimental settings. Examples include giving in dictator games, the rejection of low offers in ultimatum games, as well as the conditional cooperation and punishment of free riders in public good games. Outside the lab, the degree of inequality aversion might influence individual preferences over tax deductions, insurance take-up and many more aspects of redistribution.

Despite its prominence, the measurement of inequality aversion is still debated. While inequality aversion is intuitively appealing and successful in rationalizing observed behavior in the aforementioned games, it is unclear whether it is indeed inequality aversion or other considerations that are measured in these settings. For instance, Engelmann and Strobel (2004) show that a combination of selfishness, efficiency concerns, and maximin preferences are sufficient to explain behavior in simple distribution experiments. Inequality aversion is not needed to rationalize behavior and, moreover, is at odds with several patterns in their data. In a related paper, Fershtman et al. (2012) conclude that social norms are more successful than inequality aversion in explaining behavior in dictator and trust games. Thus, decisions in games that appear to be based on inequality aversion may actually originate from other preferences. Still, Tricomi et al. (2010) find neural evidence for the existence of inequality aversion in humans using functional magnetic resonance imaging.

Motivated by this literature, the present paper develops and implements a new method to measure inequality aversion, which we term "coupled lotteries." In this method, two players are endowed with two identical binary lotteries (offering amount x with probability $p > 0$ and nothing otherwise) and the only choice they make is whether they want to play out the lotteries independently ("separated") or with perfect positive correlation ("coupled"). This choice only affects *ex post* inequality in outcomes, i.e., there is no outcome inequality if lot-

teries are coupled and a $2p(1-p)$ chance of inequality if lotteries are separated. When only monetary payoff over both peers is maximized, not coupling the lotteries is the social planner's optimal choice, because risk could be shared *ex post* by reallocating payoffs if one party wins and the other loses. Since coupling the lotteries decreases the chance of inequality for sure and changes nothing except the possibility for *ex post* risk sharing, inequality aversion seems to be the only way to rationalize this choice.

We implement an incentivized version of our coupled lotteries game in a representative survey of about 850 rural households in Ubon Ratchathani, a province in northeastern Thailand. The advantage of using this survey is, first, that we have a more heterogeneous sample than lab experiments with students, second, that we can employ rather high monetary incentives comprising more than a half day's wage and, third, that we can use extensive socioeconomic data for each respondent. Thus, we are able to analyze the share of respondents displaying inequality aversion as well as which personal and socioeconomic characteristics are associated with inequality aversion. We test six hypotheses derived from the literature (mainly based on the structural estimation of Bellemare et al. (2008)): inequality aversion is related to being male, older, less educated, poorer, showing higher risk aversion, and having social status concerns. Additionally, we can directly use our measure to explain real-life behavior: following the theoretical considerations of Friedl et al. (2014), we test the empirical relation between inequality aversion and insurance take-up. Moreover, we investigate whether inequality aversion is related to less risky farming decisions.

We find that almost 40 percent of respondents choose to couple the lotteries versus more than 60 percent who prefer to separate. Regarding the six hypotheses stated above, our evidence supports three of them (while evidence on the three others is not robust): choosing coupled lotteries is significantly related to being male, being more risk averse, and having social status concerns, which is in line with theoretical predictions. Potential *ex post* risk sharing (which might motivate to separate) is not supported by our data, as risk sharing would be related to, for example, trust in the village. This is not compatible with our results, which show that subjects who trust more prefer to couple. Finally, we find that households with inequality averse household heads are more likely to have some kind of formal insurance and are also more likely to diversify crops (if they are farmers), which is a way to diversify risk of crop loss. This is in line with theoretical predictions on insurance take-up.

We opt for a lab-in-the-field experiment as we want to take advantage of the unique setting: interviews are conducted in each respondent's house and we let them each play coupled lotteries against one of their neighbors, who is not part of the sample. Analyzing inequality aversion within such a close reference group is rare in the economic literature and gives insights into social preferences when social distance is small. It might, however, present an upper bound of inequality aversion (e.g. Bandiera et al., 2005; Charness and Gneezy, 2008; Kranton and Sanders, 2017). Furthermore, there might be culturally determined differences in the degree of inequality aversion between our relatively poor Thai sample and samples in industrialized countries (see Alesina et al., 2011, for a general discussion on preferences for redistribution). Croson and Gneezy (2009), for example, analyzing various ultimatum and dictator games, conclude that, in general, women seem to be more inequality averse, which contrasts with our finding. However, as highlighted before, these games might not be suited for identifying inequality aversion because they exhibit a trade-off between maximizing efficiency and inequality aversion. In general, we do not have reason to believe that the cultural background affects the external validity of our method itself.

To eventually rule out risk sharing motives completely, we additionally implement our measure in a "more controlled" setting, namely, a lab with students in Germany. The anonymity and structure of a lab experiment inhibits *ex post* risk sharing. Furthermore, we investigate the relationship between coupled lotteries and a still common measure for inequality aversion, the dictator game. We find that the degree of inequality aversion in the German student sample is larger than in the Thai rural sample. This is a bit surprising given the high social distance between lab participants in comparison to neighbors, however, the two samples differ in many respects. Thus, it is comforting that we also find similar results in both samples, such as coupling being related to risk aversion and that we can confirm that women are not more inequality averse than men. However, we do not find any correlation between decisions in the coupled lotteries and the dictator game within this student population. This is in line with our claim that dictator giving is driven by different other-regarding preferences. It also speaks to previous literature that refutes the correlation of inequality aversion parameters derived from the dictator game and other simple distribution games (see Blanco et al., 2011).

Our paper is closely related to the work of Bellemare et al. (2008) who analyze inequality aversion in a representative sample of the Dutch population. Based on observed decisions in the ultimatum game and proposers' elicited expectations of

rejection rates, they estimate a structural model of decision making under uncertainty from which they derive the degree of inequality aversion. Compared to our method, they obtain a metric measure about the degree of inequality aversion and are able to distinguish between advantageous and disadvantageous inequality aversion, while we only obtain a binary measure, i.e., we can detect whether a person is inequality averse or not. However, our method has the potential advantage that we do not need to control for expectations and risk attitudes but can observe inequality aversion directly from choices. It is much easier to implement this method in large scale surveys and to obtain a relatively simple, fast, and still valid measure for the presence of inequality aversion. Furthermore, the game is easy to understand and decreases the concern of confounding preferences with decision errors.

The reason why Bellemare et al. (2008) use structural modeling lies in the aforementioned difficulty of distinguishing inequality aversion from other motives in standard distribution games. So far, probably the most common ways to measure inequality aversion are observing choices in dictator and ultimatum games (see Levitt and List, 2007). However, the literature shows that these choices can also be explained by other considerations. In the case of the ultimatum game, first, it is the fact that giving might depend on the belief a proposer has about the acceptance threshold of the responder. Already Forsythe et al. (1994) concludes that strategic considerations partly drive ultimatum giving as people give more in the ultimatum than in the dictator game. This is confirmed by Bellemare et al. (2008) as their model controlling for individual beliefs fits observations better than a model assuming rational expectations. On the responder side, rejecting an offer can be caused by inequality aversion but also by negative reciprocity (Brandts and Solà, 2001; Falk et al., 2003; Falk and Fischbacher, 2006) or other punishment motives. The active role of the responder and, thereby, the strategic interaction is eliminated in the dictator game. Here, there is especially one motive that competes with inequality aversion in explaining observed choices: (impure) altruism. Andreoni and Miller (2002) show that simple altruistic preferences explain dictator giving extremely well. This work is extended by, among others, Chowdhury and Jeon (2014) and Korenok et al. (2013), who find more support for impure altruism as there is imperfect crowding-out if recipients have own income (Bolton and Katok, 1998). In any case, it is not clear whether dictators derive a higher utility from equalizing payoffs, from the warm glow of giving, or, most likely, from both kinds of motives. This is emphasized by the finding of Engelmann and Strobel (2004) that a combination of preferences explains behav-

ior in standard distribution games better than a single social preference. Given the previous literature, we approach inequality aversion more directly: we are not investigating whether inequality aversion can explain behavior in an already existing game but we are looking for a game that can give us a consistent measure of inequality aversion.

The design of our game itself is related to a different kind of distribution games that address different correlation structures between the own and the other's risky payoffs, which, however, do not directly address inequality aversion as such. Rohde and Rohde (2011) concentrate on how correlated payoffs affect risk taking and give participants choices between problems with varying risk and correlation structures. However, neither do these choices resemble our coupled lotteries measures nor are they suited to test explicitly for inequality aversion. Bolton and Ockenfels (2010) and Adam et al. (2014) use binary decision problems with (perfect) negative correlation between payoffs to investigate the effect of social comparison on risk taking. Trautmann (2010), relying on binary distribution choices from Broome (1991), discusses how to include fairness into utilitarian welfare models.

Several paper address the empirical relevance of *ex ante* and *ex post* inequality aversion and how inequality aversion translates to risky environments in general (e.g. Brock et al., 2013; Fudenberg and Levine, 2012; Krawczyk and Le Lec, 2010; Saito, 2013). We deliberately only concentrate on *ex post* inequality and ignore *ex ante* inequality by ruling it out with our design. However, we do not neglect the existence or the possibly greater empirical relevance of *ex ante* inequality. Actually, our results support the notion that exact equality in outcomes does not seem to be important for many individuals. Given that equalizing payoffs in our game does not mean foregoing own payoff, it would not even be costly to establish equal outcomes and, still, many persons do not seem to care in both our samples. This is in line with the aforementioned literature, arguing and showing that inequality aversion in outcomes is not the main driver for the observed behavior in standard distribution games. Nevertheless, for some persons exactly equalizing payoffs does matter and we can show this is informative about real-life behavior.

Hence, we not only contribute to the literature by deriving a method to measure inequality aversion in outcomes but also by analyzing who is actually inequality averse and how inequality aversion is related to economic behavior, like insurance take-up or crop portfolio choices. The literature on the relation between inequality aversion and real-life decision making is especially scarce. However,

knowing about areas of life that might be affected by inequality aversion and knowing about who is affected is necessary for deriving an empirically validated microfoundation of inequality aversion. It could inform policy makers thinking about implementing inequality-reducing policies like progressive taxes or social benefits.

The remainder of the paper is organized as follows. The subsequent Section 2.2 introduces the theoretical background and derives hypotheses. Methods and data are presented in Section 2.3, while Section 2.4 provides results. Section 2.5 illustrates the relation between our coupled lotteries measure and insurance take-up. Section 2.6 discusses the supplemental lab experiment and the additional insight into the relation between our measure and dictator giving. Robustness checks are shown in Section 2.7. Section 2.8 concludes.

2.2 Theoretical Background

2.2.1 Coupled Lotteries

Suppose there are two players, i and j, who are both endowed with a lottery L each. With a probability of $p > 0$, this lottery yields x and with a probability of $1 - p$ nothing. The only decision players have to make is whether they want to keep their lotteries separate, where it is independently drawn for each player if she loses or wins, or whether they want their lotteries coupled, in which case a single draw determines if both lose or both win. For simplicity, let us assume that $x = 100$ and $p = 0.5$ (see Figure 2.1).

Player 2 \ Player 1	Separ.	Coupl.
Separ.	A	C
Coupling	C	B

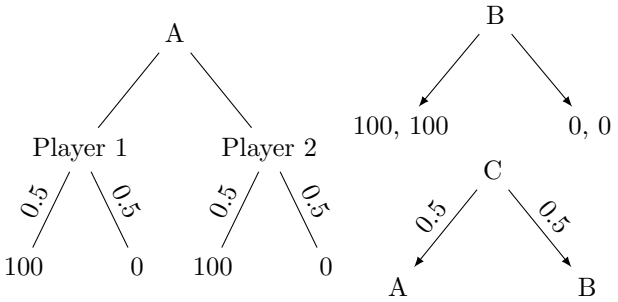


Figure 2.1: Coupled Lotteries: Decision Matrix and Connected Outcomes

In standard decision theory, subjects care only about their own payoffs. Therefore, they should be indifferent between coupling and separating their lotteries, as in both cases the chance of winning is 50%. One alternative to this pure self-interest hypothesis is inequality aversion. Like Bellemare et al. (2008), we employ

the model of Fehr and Schmidt (1999) to model inequality aversion. Suppose that the payoffs of i and j are given by x_i and x_j respectively. Then, the utility of i, V_i , is given by

$$V_i = x_i - \alpha_i \max\{x_j - x_i, 0\} - \beta_i \max\{x_i - x_j, 0\} \tag{2.1}$$

where $0 \leq \beta_i < 1$ and $\alpha_i \geq \beta_i$. Inequality aversion implies $0 < \beta_i < 1$ and $\alpha_i \geq \beta_i$.

For coupling the lotteries, we always have $x_i = x_j$. In contrast, when separating, there is a $2p(1-p)$ chance of unequal outcomes.[1] Given our parametrization this means, that coupling yields an expected payoff of

$$E(V_i(coupling)) = 50. \tag{2.2}$$

Separating entails a 1/4 chance of being better off than the peer as well as another 1/4 chance of being worse off than her. Consequently, we obtain

$$E(V_i(separating)) = 50 - 0.25\alpha_i 100 - 0.25\beta_i 100, \tag{2.3}$$

This means that inequality averse subjects, whose β and α are strictly greater than 0, should prefer coupling. Note that this conclusion also holds in the case of risk aversion or any non-linearities in the perception of inequality, i.e. we can replace V_i in equation (1) also by the more general form:

$$V_i = u_i(x_i) - \alpha_i v_i(\max\{x_j - x_i, 0\}) - \beta_i v_i(\max\{x_i - x_j, 0\}), \tag{2.4}$$

where u_i reflects the risk attitude and the strictly increasing v_i with $v_i(0) = 0$ reflects the perception of inequality. Moreover, β_i could be even negative, i.e. the subject actually likes advantageous inequality, as long as its absolute value is less than α (the parameter for disadvantageous inequality). However, in contrast to Bellemare et al. (2008), our method does not separate disadvantageous inequality aversion from an advantageous one.

[1] Note, that, independent of coupling or separating, it is always the case that $E(x_i) = E(x_j)$. That means there is no inequality in opportunity or *ex ante* inequality. Subjects who only care about expected outcomes are hence indifferent between coupling and separating. However, subjects who care about outcome inequality consider the **expected difference** in outcomes. Therefore, we apply the expected utility function on Fehr-Schmidt preferences and not Fehr-Schmidt preferences on expected outcomes. In general, Fehr-Schmidt preferences are not well suited to model *ex ante* and *ex post* fairness for risky outcomes (see Fudenberg and Levine, 2012).

2.2.2 Hypotheses

Despite the differences between our method and that of Bellemare et al. (2008), we believe that both methods measure the same preference. Our initial hypothesis is that the correlation structure between sociodemographic characteristics and inequality aversion in our Thai subject pool is similar to that of the Dutch subject pool. Thus, according to the results of Bellemare et al. (2008), we obtain the following hypotheses:

Hypothesis 1: *Male subjects are more inequality averse and, thus, they are more likely to couple.*

In contrast to the previous literature, as for example discussed in Croson and Gneezy (2009), Bellemare et al. (2008) find no significant sex difference in disadvantageous inequality (α), but they find that being male is related to a larger β.

Hypothesis 2: *Older subjects are more inequality averse and, thus, they are more likely to couple.*

Hypothesis 3: *Less educated subjects are more inequality averse and, thus, they are more likely to couple.*

Hypothesis 4: *Poorer subjects are more inequality averse and, thus, they are more likely to couple.*

In addition to those sociodemographic variables analyzed by Bellemare et al. (2008), there is evidence that inequality averse subjects are also risk averse (Carlsson et al., 2005). Thus, we include the following hypothesis:

Hypothesis 5: *More risk averse subjects are more inequality averse and, thus, they are more likely to couple.*

Furthermore, since social status concerns can be a driver of inequality aversion (Shaw and Olson, 2012), a measure for social status concerns is included in the survey. Our last hypothesis reads as follows:

Hypothesis 6: *Subjects with social status concerns are more inequality averse and, thus, they are more likely to couple.*

2.2.3 Alternative Explanations

***Ex Post* Risk Sharing.** For inequality neutral individuals, there is no payoff gain in coupling the lotteries. However, in our household-survey-setting there might be an individual gain in choosing to separate the lotteries due to the possibility of *ex post* risk sharing. In fact, separating the lotteries is the optimal choice from a social planner perspective as payoffs can be redistributed if one party loses and the other wins. Since our survey participants live next to each other, it is hard to control whether they might share the money afterwards as well. We think that it is a strength of our main analysis that subjects know each other well, implying that the social comparison should have a much stronger impact than in an anonymous laboratory setting. Still, we make sure that after introducing the game, subjects had no opportunity to interact before making their decisions, i.e., they were not able to agree *ex ante* on *ex post* risk sharing. Nevertheless, the possibility of *ex post* risk sharing is a potential confound when analyzing inequality aversion with our survey data. We subsequently address this potential confounding factor by adding variables to the analysis that help us to detect the possible presence of *ex post* risk sharing. Greater trust and social connection between the respondent and her neighbor should increase the incentive for *ex post* risk sharing, thereby increasing the probability of separating. This is because greater trust decreases the level of betrayal aversion (Bohnet et al., 2008; Bolton and Ockenfels, 2010). Bohnet et al. (2008, p. 294) describe betrayal aversion as being, "less willing to take a risk when the source of the risk is another person rather than nature." In our setting nature decides the outcome of the lottery, but the neighbor decides to share the money afterwards or not. Thus, she is a source of risk that induces betrayal aversion. In our additional laboratory experiment (like in any other lab setting), the possibility to share risk *ex post* can be ruled out as participants usually do not know each other, cannot communicate during the session, and leave the laboratory directly after the private payout. This means that inequality neutral subjects should be indifferent between coupling and separating in the lab.

Reciprocity and Altruism. Even if behavior in experimental games cannot fully be rationalized by efficiency or maximin preferences, there might be other social preferences that confound the measurement of inequality aversion, e.g.

reciprocity and altruism. Reciprocity is the preference to reward kind and punish unkind actions (Falk and Fischbacher, 2006). In our setting there is no possibility to behave reciprocally, since respondents neither can influence their own or the probability of their neighbors winning nor can they see the choice their neighbors make (and vice versa). Fehr and Fischbacher (2003, p. 785) define altruism in the behavioral economics sense "as being costly acts that confer economic benefits on other individuals." Except for *ex post* risk sharing, which we discuss extensively in this paper, there is no scope for altruism in our game.

2.3 Methodology

2.3.1 Survey Design

The survey including the coupled lotteries game was conducted in rural Thailand in 2014. It is part of an add-on project to a panel survey in rural Thailand and Vietnam starting from 2007 that is now known as the Thailand Vietnam Socio Economic Panel (TVSEP). At the time our game was played, 4 full waves had been completed (2007, 2008, 2010, and 2013).

Each survey wave consists of a household and a village questionnaire. The household questionnaire is tailored to the lives of families in rural areas, largely engaged in agricultural business. It includes comprehensive sections on crop farming, livestock rearing, borrowing/lending as well as saving decisions, health, various socio-economic items for every household member, and, in particular, questions on exposure to shocks and anticipated risks. In some waves, personal opinions on topics such as inequality and trust are sought. The full sample consists of about 4400 rural households in 440 villages over six provinces in Thailand and Vietnam. It is representative for the rural populations in these two countries and deliberately excludes households living in urban areas. The three-stage sampling procedure is described in Hardeweg et al. (2013).

The add-on aims at analyzing individual risk preferences in rural populations in more detail, especially how these preferences vary in different situations and how they interact with skills like numeracy and financial literacy. The corresponding questionnaire is substantially shorter than a full TVSEP survey. Furthermore, it is conducted in only one of the survey provinces in Thailand, Ubon Ratchathani, which borders Cambodia and Laos (see Figure 2.2). The province is located in the northeastern part of Thailand ("Isan"), which is the poorest region in the country and where most households engage in agricultural activities. The full sample comprises about 850 individuals/households.

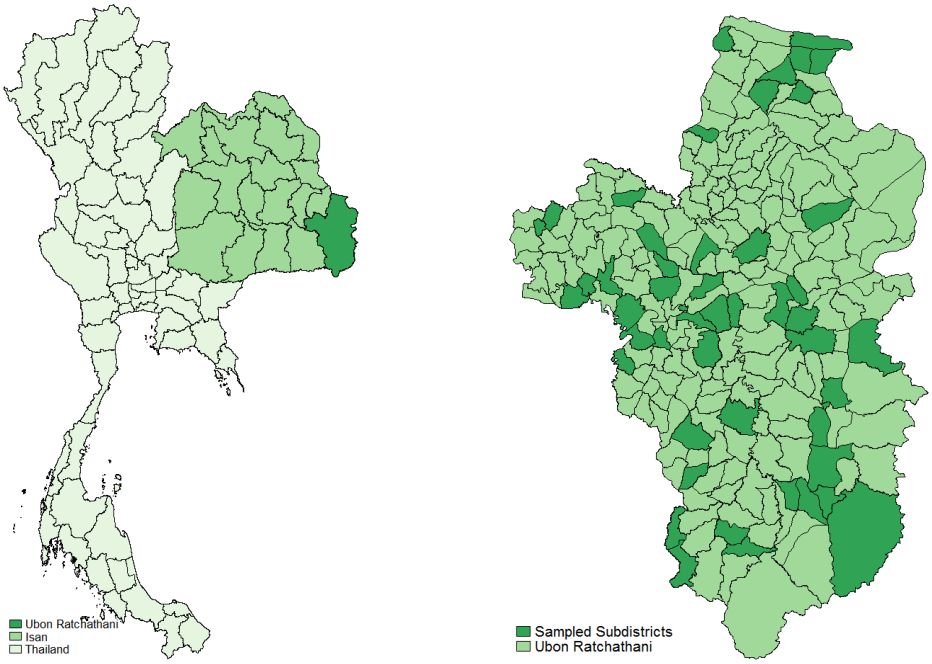


Figure 2.2: Study Site, Ubon Ratchathani and Sampled Subdistricts

For most specifications, we combine 2014 and 2013 data, as it results in a richer dataset and gives us the variables we are mainly interested in. The 2013 data include measures on trust and social comparison as well as the socio-economic variables we seek to analyze. We do not exploit the panel structure but include variables that we assume to be stable over the two points in time. Thus, we specify our regressions as a one-period model. If we want to exploit information on social comparison and trust, we can only do so if an individual answered the questionnaires in 2014 and in 2013, because this is personal information that cannot be extrapolated from the answers of another household member. Unfortunately, some respondents only answered the 2014 survey. This results in a smaller sample size for some of the regressions; this subsample consists of 521 individuals. Other variables from 2013, like sex and education, are available as long as the 2014 respondent was already part of the household in 2013, which is always the case.[2]

2.3.2 Coupled Lotteries in the Field

Coupled lotteries is one of four short "games" played toward the end of the survey. The preceding section asks respondents to answer a battery of items that

[2] In most cases the respondent in both years is the household head or their spouse.

measure their financial literacy. The following and final part of the survey is a quiz to measure numeracy. Thus, the games are surrounded by two tasks that ask for similar cognitive and computational skills. However, the financial literacy and numeracy sections are not designed to assess any sort of preference but are knowledge-based.

Respondents are explicitly asked for consent to participate in the games, which are not part of the general survey (see Appendix Material A.2.1). Since none of the respondents declines to play, no concern for sample selection into the games is given. In the questionnaire, coupled lotteries is titled "social game" and is played after the three other games.[3] The whole task is incentivized by randomly choosing one game to be implemented for payoff. On average, respondents earn 165 THB (ca. 3.70€) including a participation fee of 30 THB (ca. 0.68€), which is more than a half day's wage.

Coupled lotteries is played out by gaining either 100 THB or 0 THB, each with 50% probability determined by a coin flip (see Appendix Material A.2.2). The survey respondents are assigned the role of player i, which is a trivial assignment as roles are symmetric. We ask a neighbor of each respondent to join the game as player j. Since the survey is conducted in small villages with mostly about 100 inhabitants, players know each other quite well. However, the neighbors are not part of the survey sample and, therefore, we do not have further data on them.[4] The crucial decision that both players then make is deciding whether their lotteries are played out by one coin flip for each of them or by one coin flip together. Thus, they have to decide whether they want their risk to be perfectly positively correlated to or to be independent of their peer's risk. As explained before, the expected total payoff is equal in both cases, but the correlation structure changes the relative payoff. Both participants, although sitting next to each other, have to indicate their choice secretly on a piece of paper that is collected by the interviewer who then flips the coin(s) and announces the outcome (see Appendix Material A.2.3).[5] If both players decide to couple, so that only one coin is flipped, both either receive 100 THB or nothing. If both decide to separate, one coin is flipped for each of them and both receive their respective payoff, independently

[3] The first is a multiple price list to elicit risk preferences following Holt and Laury (2002), the second consists of four multiple price lists to elicit time preferences, and the third one is the "cheating game" by Fischbacher and Föllmi-Heusi (2013). We have no reason to believe that these games themselves prime behavior in our game in any specific way. Importantly, the first two games are only played out after the coupled lotteries game and reported numbers in the cheating game are uncorrelated to decisions in the coupled lotteries game.

[4] Except for four observations, we could always find a neighbor who agreed to participate.

[5] As mentioned before the respondent is paid for only one of the games, but the neighbor is paid directly after the social game has been played.

from each other. If they have conflicting preferences meaning one decides to couple and the other to separate, an additional coin is flipped to determine how the lottery is played out (see again Figure 2.1). To avoid experimenter demand effects, we neither encourage nor discourage participants with respect to *ex post* risk sharing.

2.3.3 Empirical Approach

Decisions about coupled lotteries are binary and since every participant makes a valid choice, we use a simple logit model to estimate our regressions. Let Y_i denote the decision a participant takes, where $y_i = 0$ means separating the lotteries and $y_i = 1$ means coupling the lotteries. The first regression specification only contains information available for the full sample and reads as follows:

$$Y_i = \alpha + X'_{it}\beta_1 + u_{it} \tag{2.5}$$

Errors are clustered on the district level and the regressor matrix X'_{it} contains sex, age, education, annual per capita log consumption, district fixed effects, and a measure for risk preference. We use consumption instead of income because, in general, it is believed to be a more reliable measure in this kind of setting. However, our results are also robust to using annual per capita income (see Section 2.7). As the survey was designed to analyze risk preferences, various measures of risk taking are available. These are correlated with each other but seek to measure risk taking behavior in different situations. We include risk preference measured through the multiple price list method (inspired by Holt and Laury, 2002), which is the only incentivized elicitation method we employed. Education is measured by years of schooling. In one of the regressions we include a first measure to test for *ex post* risk sharing. To do so a proxy for trust or closeness to people in the village is used. We employ a variable that indicates whether respondents receive agricultural advice from their neighbors or relatives.

The second regression specification applies to the subsample of respondents who played the game in 2014 and answered the survey in 2013:

$$Y_i = \alpha + X'_{it}\beta_1 + S'_{it}\beta_2 + u_{it} \tag{2.6}$$

Besides the same regressor matrix X'_{it} as in 2.5, matrix S'_{it} is added. S'_{it} contains our measures for social status concerns and a more accurate measure of trust.

Respondents are asked whether they compare their standard of living to other persons and to whom. They can choose between 11 different reference groups, where one option is "Neighbors" and another one is "I don't compare myself to anyone." We construct two dummies that equal 1 when respondents choose the respective option and 0 otherwise. Thus, we have a dichotomous measure whether respondents "keep up with the Joneses" (choose their neighbors as main reference group for social comparison) and whether respondents care about social status at all. As indicated, respondents can only choose one reference group. Hence, a 0 in the "neighbor dummy" does not mean that respondents do not regard their neighbors for social comparison, but that neighbors are not their primary group for social comparison. In that sense our other measure for general social status concerns is cleaner, which is why we primarily focus on whether respondents compare their standard of living at all in the later analysis. A 4-point Likert scale measures how much respondents trust other people living in their village, such that we have a more reliable measure for the possibility of *ex post* risk sharing.

2.3.4 Descriptive Statistics

Table 2.1 shows summary statistics of the variables used in the two regression models. We include respondents between the age of 16 and 85 for the analysis. The descriptives are split by whether the respondent is the same in both waves, 2013 and 2014, or not. There are two significant differences between the two subsamples, i.e. age and education. As these two variables are highly correlated in our sample, it is not surprising that there is a significant difference in education given that there is one in age. The difference in age can be explained by the fact that the add-on project in 2014 put less priority on interviewing the household head than did the main project and that other household members are, on average, younger than their head. Nevertheless, the fraction choosing to separate is not significantly different between those respondents who only answer the 2014 questionnaire and those who participate in both 2013 and 2014 (see Appendix Figure A.1.1). Overall, these statistics indicate that we can concentrate on the latter subsample and still receive valid results for the whole sample.

Table 2.1: Descriptive Statistics of the Sample

	Full Sample	2013 and 2014	Only 2014	Difference
Coupling	0.37	0.36	0.39	0.03
Male	0.37	0.35	0.40	0.05
Age	52.97	55.24	49.40	−5.84***
Years of Schooling	5.94	5.48	6.66	1.18***
Ann. Consumption per Cap.	716.57	724.36	703.43	−20.92
Risk Taking, MPL	11.03	10.98	11.11	0.13
Advice from Neighbor	0.14	0.14	0.13	−0.02
Trust in Village		3.12		
Comparing with Neighbors		0.48		
Not Comparing		0.16		
Observations	851	521	330	851

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

From the table, it can be seen that the majority prefers to separate. Given the structure of our sample (more women and small degree of risk aversion), this is not surprising based on our hypotheses. Around 60% of the respondents are female and the fraction of women who choose to separate is significantly higher (two-sided t-test, $p < 0.05$) than the fraction of men who decide to do so (see Appendix Figure A.1.2). Furthermore, the rural Thai population seems, on average, to be more risk seeking than the Western populations previously studied. Dohmen et al. (2011), for example, use the same multiple price list in a representative German sample. Not only is the mean for risk taking higher in the Thai sample, but the whole distribution is skewed to the right with a much higher proportion never switching from the lottery to the safe option (see Appendix Figure A.1.3). This result is in line with the conclusion of l'Haridon and Vieider (2019) that, on average, persons in poorer countries are substantially more risk tolerant than persons in rich countries. Years of schooling and annual consumption per capita in the Thai rural population are substantially lower than in samples from industrialized countries. Turning to the social comparison variables, almost half of the sample states that the main group they are comparing with are their neighbors. Besides this group, there are around 16% of respondents who do not compare themselves to anyone. Thus, social comparison motives between players have an important role in our setting.

2.4 Results

For each regression, we report average marginal effects (AME). Hence, for each observation all variables, except the one for which the effect is estimated, are held at their realized level. The AME is obtained by taking the mean over all individual marginal effects. Given the many dummy variables, estimating marginal effects at the means (MEM) would be less meaningful as a dummy value between 0 and 1 cannot be interpreted economically (such as being 80 percent female). Table 2.2 shows regression results using model specification 1, i.e. analyzing the full sample based on equation 2.5. First, the variables for testing hypothesis 1-5 are included in two steps and then, in the third regression we add our proxy variable for trust between neighbors to test for the relevance of *ex post* risk sharing.

Table 2.2: Logistic Regressions, Full Sample

	(1)	(2)	(3)
Male	0.101***	0.099***	0.099***
	(0.032)	(0.033)	(0.034)
Age	−0.002**	−0.001	−0.001
	(0.001)	(0.001)	(0.001)
Years of Schooling	0.003	0.004	0.005
	(0.005)	(0.005)	(0.005)
Ann. Consumption per Cap. (log)		−0.015	−0.013
		(0.026)	(0.025)
Risk Taking, MPL	−0.004*	−0.004*	−0.004*
	(0.002)	(0.002)	(0.002)
Advice from Neighbor			0.075*
			(0.039)
Observations	849	829	829
Baseline Predicted Probability	0.369	0.367	0.367
McFaddens R-squared	0.046	0.046	0.048

Dependent Var.: Decision to separate (=0) or couple (=1). Average marginal effects reported. SE clustered at district level. District dummies not reported.
* p < 0.10, ** p < 0.05, *** p < 0.01

The highly significant positive effect on being male is in line with hypothesis 1; on average men are 10 percentage points more likely to couple the lotteries than women. This effect is in line with Bellemare et al. (2008) but stronger than their coefficient for the Dutch population. Consequently, we cannot confirm previous

studies that find women to be more inequality averse than men. One explanation could be cultural differences; Croson and Gneezy (2009) mention that cultural biases could cause sex differences in preferences. Still, from this perspective, it may be surprising that the results in Bellemare et al. (2008) point in the same direction as ours. Another explanation could be that, in our game, there is no trade-off between inequality and efficiency, as in some previous studies. If men prefer efficiency over inequality, this does not automatically mean that they are less inequality averse than women. In general, there might be several confounds in previous studies as to why women appear to be more inequality averse than men. We do not confirm hypotheses 2, 3 and 4. Age is only significant in regression (1) and the effect goes in the opposite direction as hypothesized. Education and consumption are never significant. These insignificances are not related to a possible multicollinearity problem. Preference for risk taking is significant in the expected direction. The effect size is not as small as it seems at first glance given that the risk taking variable takes values between 1 and 21. Going from 1 to 21 decreases the probability of coupling the lotteries by over 17 percentage points. Thus, we confirm hypothesis 5, that risk aversion and inequality aversion are positively related to each other. Another possibility would be that risk taking is not related to less inequality aversion in general, but to favoring advantageous inequality. In our setting, only choosing to separate can lead to advantageous inequality and more risk prone individuals might want to get ahead of the Joneses and are less afraid of falling behind. However, this argument is not supported by our results for social status concerns (see the following paragraphs). Furthermore, there is no significant interaction effect between risk and social comparing.

Regarding *ex post* risk sharing, we clearly find evidence against this alternative explanation. Respondents who are closer to their neighbors are more likely to couple lotteries instead of separating and potentially sharing afterwards. This effect is significant and large.

We now turn to the subsample of 521 individuals for whom we have information about the importance of social status concerns as well as the improved measure of trust. In Table 2.3, we report results using model specification (2), i.e. equation 2.6. Regression (1) contains the same variables as regression (2) of Table 2.2 to check how consistent the model is estimated with the smaller sample. In regressions (2), (3), and (4) social status and trust variables are added. The effect sizes for sex are smaller in all presented regressions but are still large and significant. Age and consumption are insignificant as before, which is intuitive,

because possible effects should have already been significant in the larger sample. For education, however, we find significant effects. Due to the significant differences in age and education between the two samples and the small sample size here, we do not want to interpret this as evidence for a sizable effect of education as stated by hypothesis 3. If there is any effect, more educated individuals seem to be more inequality averse, not the other way around.

Table 2.3: Logistic Regressions, Subsample

	(1)	(2)	(3)	(4)
Male	0.067*	0.068*	0.071**	0.068**
	(0.037)	(0.038)	(0.037)	(0.035)
Age	−0.000	−0.000	−0.000	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Years of Schooling	0.015**	0.015**	0.013*	0.013*
	(0.007)	(0.007)	(0.007)	(0.007)
Ann. Consumption per Cap. (log)	−0.031	−0.033	−0.035	−0.032
	(0.034)	(0.034)	(0.034)	(0.033)
Risk Taking, MPL	−0.003	−0.004	−0.004	−0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Comparing with Neighbors		0.060*		
		(0.036)		
Not Comparing			−0.127**	−0.129***
			(0.048)	(0.045)
Advice from Neighbor				0.086*
				(0.048)
Trust in Village				0.059**
				(0.025)
Observations	521	521	521	521
Baseline Predicted Probability	0.359	0.359	0.359	0.359
McFaddens R-squared	0.044	0.047	0.052	0.061

Dependent Var.: Decision to separate (=0) or couple (=1). Average marginal effects reported. SE clustered at district level. District dummies not reported.
* p < 0.10, ** p < 0.05, *** p < 0.01

Unfortunately, the smaller sample size turns the coefficients on risk taking insignificant. Still, they do not differ in direction and size from the full sample specification. Looking at the measures for social status concerns, we cannot reject hypothesis six. Having the neighbors as main reference group for social

comparison increases the probability of coupling while having no social status concerns decreases the probability of coupling. The coefficient for having no social status concerns is large and significant at the 5% level. Moreover, our finding indicates that inequality aversion seems to be stronger if the other person is part of the individual's main reference group for social comparison. We cannot use both variables in the same regression because a substantial part of the sample compares themselves to neighbors, which makes the dummies per definition highly correlated. Therefore, they are included separately in regressions (2) and (3).

Eventually, we again test for *ex post* risk sharing, this time using the proxy from the 2014 survey and adding a more reliable measure on trust from 2013. Both coefficients depict a sizable effect and are clearly not in favor of *ex post* risk sharing. Respondents trusting other people in the village a lot are 16 percentage points more likely to couple the lotteries than respondents who do not trust other villagers at all. This is in line with inequality aversion increasing in reduced social distance and that the "comparing with neighbors" dummy turns insignificant if the two variables for trust are added.

2.5 Inequality Aversion and Field Behavior

In order to test the "predictive power" of our measure for inequality aversion, we estimate the correlation between the measure and actual behavior that theoretically should be related to inequality aversion. More precisely, we run regressions to analyze whether our measure is related to formal insurance take-up as well as means taken to reduce the risk of grown crop portfolios, which could be regarded as some kind of informal insurance take-up. Friedl et al. (2014) show theoretically that insurance take-up reduces inequality among peers facing similar risks as long as risks are not perfectly positively correlated between them. Therefore, inequality averse subjects should be more prone to insurance take-up, which we want to test in our field setting.

Table 2.4 reports the regression results for different kinds of insurance take-up. Using the TVSEP 2013 survey data, we construct a dummy indicating whether the household has any kind of voluntary, formal insurance.

Table 2.4: Logistic Regressions, Insurance Demand and Crop Diversity

	Having Insurance	Above Mean Crop Index	Glutinous Rice	Cassava
Inequality Aversion	0.066**	0.102*	−0.078	0.086**
	(0.027)	(0.060)	(0.054)	(0.046)
Male	−0.038	−0.002	0.022	0.000
	(0.032)	(0.084)	(0.053)	(0.048)
Age	0.003	0.002	−0.001	−0.002
	(0.003)	(0.002)	(0.002)	(0.002)
Years of Schooling	0.013	−0.001	−0.008	−0.001
	(0.012)	(0.010)	(0.006)	(0.007)
Ann. Cons. per Cap. (log)	0.079***	−0.047	0.043	−0.056
	(0.028)	(0.056)	(0.033)	(0.043)
Household nucleus size	0.048***	−0.013	0.017	−0.016
	(0.014)	(0.013)	(0.015)	(0.014)
Risk Taking, MPL	−0.000	−0.000	0.001	−0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
Numeracy	−0.002	0.017	0.007	−0.017
	(0.018)	(0.015)	(0.016)	(0.017)
Financial Literacy	0.009	−0.005	−0.012	0.000
	(0.022)	(0.028)	(0.025)	(0.017)
Self-Employed	0.070			
	(0.075)			
HH better off in 5 years	−0.038*			
	(0.021)			
Number Anticipated Risks	−0.007			
	(0.009)			
Number Weather Risks		0.018	0.026	0.018
		(0.036)	(0.022)	(0.019)
Land Size		0.005	0.007**	0.007***
		(0.004)	(0.003)	(0.001)
Number of Tractors		0.091*	0.092***	−0.014
		(0.048)	(0.032)	(0.045)
Number of Waterpumps		−0.021	−0.017	−0.070**
		(0.048)	(0.025)	(0.027)
Observations	418	319	296	288
Baseline Predicted Probability	0.767	0.537	0.774	0.217
McFaddens R-squared	0.123	0.080	0.193	0.334

Dependent Var.: Having insurance, Simpson index of crop-land share diversification above mean and growing glutinous rice and cassava. Average marginal effects reported. District dummies not reported. SE clustered at district level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Furthermore, we run a regression using the Simpson Index of (crop) Diversification as used in Nguyen et al. (2017) for the same households; however, only for those households engaged in own agricultural activities. Crop diversification is interpreted as informal insurance (see Skoufias, 2003), because it diversifies the risk of crop loss, especially if the additionally planted crops are less sensitive to weather shocks or pest infestation. Farmers in Northeastern Thailand traditionally grow glutinous rice, which is also their main staple. Cassava is a crop that yields similar profit and fulfills the conditions to diversify crop loss. Therefore, we estimate separate regressions for growing glutinous rice and cassava to see whether the motives to grow these two differ from each other. Since the decision what risk to insure and which plants to grow are decisions that are usually made at the household level, and not the individual level, in our setting, we only include subjects who are supposed to be the main decision maker in their household, the household heads.

Our exogenous variable of interest is the decision subjects make in our coupled lotteries game. Additionally, we include controls that are hypothesized to be decisive for each specific take-up (see for example Nguyen et al., 2017), in particular our incentivized measure of risk aversion. As can be seen, respondents who are inequality averse, i.e. those who couple the lotteries, are significantly more likely to have formal insurance in their households and to have an above-average crop diversification index. The above-average diversification seems to be driven by growing cassava which, in contrast to glutinous rice, is significantly correlated with being inequality averse. Thus, we can show that our measure captures real-life decisions that are likely driven by inequality aversion.

2.6 Supplemental Lab Evidence

To gain further insights, we also ran a lab experiment in Germany including the coupled lotteries game. Our motivation for this experiment is twofold. First, besides substantially increasing social distance, because lab participants typically do not know each other, unlike neighbors in a small Thai village, and because participants in fact do not know who their counterpart for playing the game will be, taking the game to the lab inhibits *ex post* risk sharing for the very same reasons. Hence, in comparison to the field study, our lab experiment is more controlled as we can fully rule out risk sharing motives. Second, we aim to analyze

how decisions in the coupled lotteries game relate to a common (but flawed) measure for inequality aversion by additionally playing the dictator game.[6]

The sessions were conducted at the University of Kiel in March 2019. In total, 76 students participated in 2 sessions. On average the sessions lasted about 35 minutes and participants earned 13€. Besides a questionnaire, the experiment includes four games in fixed order, a dictator game, the coupled lotteries game, and two multiple choice lists eliciting the certainty equivalents of a lottery where you win(lose) 10€ or nothing with equal probabilities. Instructions for the experiment were given on the screen and in written form (see Appendix Material A.2.5). Participants were invited via hroot (Bock et al., 2014) and the experiment was programmed in oTree (Chen et al., 2016).

Descriptive statistics are shown in Table 2.5. Somewhat surprisingly, we find that in contrast to our rural Thai sample, the majority of our German student sample couples the lotteries. About 60% decide to couple the lottery and again the share is statistically significantly different from being random.

Table 2.5: Descriptive Statistics of the Lab-Sample

	Full Lab-Sample	Separators	Couplers	Difference
Male	0.42	0.45	0.40	0.05
Age	24.61	25.68	23.87	1.81*
Risk Taking, MPL	10.46	11.16	9.98	1.18
Advice from Classmate	0.51	0.45	0.56	−0.10
Dictator Keeping	6.41	6.39	6.43	−0.05
Donation Dummy	0.64	0.68	0.62	0.06
Donation Amount	45.11	38.90	49.38	−10.47
Observations	76	31	45	76

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Furthermore, we find no difference for female and male participants, which might be due to the rather homogeneous student sample in general (see Table 2.6 (1)). Still, we again cannot confirm previous findings, which are also mostly based on student samples, that women are more inequality averse than men. We find

[6] In our version of the dictator game, both parties decide how much to give to the other party and a random draw decides whose decision is going to be implemented.

a significant negative relationship between age and coupling the lotteries, which is similar to the Thai sample. However, given the small age range and the rather small sample size, insight into this relationship is limited. Our elicitation of risk preferences, using a multiple price list similar to the one in Thailand, shows a significant positive relation between risk aversion and coupling. In comparison to Thailand, the relation in Germany is stronger and more robust.

Table 2.6: Regressions, Lab Sample

	Coupling	Donating	Donating
Male	−0.042	0.075	0.051
	(0.072)	(0.119)	(0.144)
Age	−0.022***	−0.022**	−0.024***
	(0.006)	(0.009)	(0.003)
Risk Taking, MPL	−0.013**		
	(0.005)		
Advice from Classmates	0.081***		
	(0.002)		
Dictator Keeping	0.012	−0.034*	
	(0.025)	(0.015)	
Inequality Aversion			−0.035
			(0.288)
Relative Income		−0.084	−0.082
		(0.060)	(0.071)
Compare Performance		0.260***	0.239*
		(0.044)	(0.124)
Happiness		0.089	0.084
		(0.103)	(0.122)
Observations	76	76	76
Baseline Predicted Probability	0.592	0.645	0.645
McFaddens R-squared	0.050	0.098	0.084

Dependent Var.: Coupling - Decision to separate (=0) or to couple (=1); Donating - Dummy for making donations to any charity; Average marginal effects reported. SE bootstrapped and clustered at session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Moreover, we asks students whether they regularly receive advice concerning their studies from classmates. This question is chosen to mimic the agricultural-advice question in our Thai sample. In line with our previous results, students

who regularly receive advice are more likely to couple. Thus, even though social distance in the lab is *ex ante* assumed to be higher, this finding supports the notion that less social distance is related to greater inequality aversion.

Importantly, we do not find a correlation between decisions in the dictator game and coupled lotteries. Both separators and couplers keep on average about 6.40€ in the dictator game. For us, this is not worrisome, as the main motivation for this paper is that the dictator game is a flawed measure for inequality aversion. Dictator giving is driven by various motives that blur the elicitation of inequality aversion in outcomes, as the literature shows. Blanco et al. (2011), for example, find no correlation between inequality aversion parameters derived from the dictator game and other distribution games. Along the same lines, one motive for donating to charities might be inequality aversion; however, it is probably not the most crucial motive. Similar to charitable (dictator) giving in the lab, the same various motives might drive charitable giving in real life. We elicit donation behavior in our questionnaire and, in fact, we find no evidence for our coupled lotteries measure to be predictive for whether people donate money to charities or not. Dictator giving, however, is significantly positively related to donating at the extensive margin (see Table 2.6 (2-3)).

2.7 Robustness Checks

In order to challenge our main results as shown in Section 2.4, we perform four kinds of robustness tests. (i) In a first step, we run regressions focusing on the socio-demographic variables sex and age. (ii) Then, we use different measures for risk preference and income. (iii) Subsequently, we change the level for clustering the error terms from district level to sub-district and village level to see how a less conservative clustering affects the results. (iv) Finally, we control for additional skills that could promote *ex post* risk sharing, numeracy and financial literacy.

Socio-demographics. We run regressions separated by sex to check for interaction effects between sex and other independent variables (see Table 2.7). In the same table, we narrow the age group we look at. Beside the estimates in Section 2.4 for respondents between 16 and 85 years, we run a regression for respondents aged 18 to 65 separated by using the two measures for social comparison. Looking at the regressions separated by sex, we do not find new significant results. Risk taking loses its significance in the small subsamples as before. However, the different size of the coefficients indicates that the relation between risk aversion

and inequality aversion is mainly driven by the male respondents. Comparing oneself with neighbors is significant for the female sample, however, having no social status concerns is not. For the male sample it is the other way around, suggesting different channels between social status concerns and inequality aversion for women and men in our sample.[7] The narrowed age group increases the effect of risk taking on the decision: although sample size is small, it turns significant. All other results are unchanged except for receiving advice from neighbors, which turns insignificant.

Table 2.7: Logistic Regressions, Split by Sex and Narrower Age Group

	Female	Male	Age 18-65	Age 18-65
Male			0.085**	0.089**
			(0.038)	(0.038)
Age	−0.000	−0.000	0.001	0.000
	(0.003)	(0.003)	(0.002)	(0.002)
Years of Schooling	0.015	0.018	0.017**	0.016**
	(0.010)	(0.014)	(0.007)	(0.007)
Ann. Consumption per Cap. (log)	−0.027	−0.056	−0.027	−0.029
	(0.034)	(0.056)	(0.033)	(0.034)
Risk Taking, MPL	−0.001	−0.007	−0.006**	−0.006**
	(0.004)	(0.005)	(0.002)	(0.002)
Comparing with Neighbors	0.080*		0.014	
	(0.048)		(0.040)	
Not Comparing		−0.261**		−0.104**
		(0.078)		(0.046)
Advice from Neighbor			−0.017	−0.016
			(0.032)	(0.034)
Trust in Village			0.067**	0.066**
			(0.031)	(0.029)
Observations	341	178	414	414
Baseline Predicted Probability	0.343	0.382	0.353	0.353
McFaddens R-squared	0.064	0.095	0.067	0.072

Dependent Var.: Decision to separate (=0) or couple (=1). Average marginal effects reported. SE clustered at district level. District dummies not reported.
* p < 0.10, ** p < 0.05, *** p < 0.01

[7] Furthermore, there is another difference between female and male respondents. Whereas trust is positively significant for coupling the lotteries for females, receiving advice from neighbors is no longer significant; with it the other way around for males.

Different measures. In this paragraph, we only report regressions using the full sample as all findings can be transferred to the sub-sample. Replacing annual consumption per capita with annual income per capita (see Table 2.8 (1)) does not change any of the other coefficients. Like consumption, income is not significant. The same holds true if consumption is replaced by the total value of durable assets the household of the respondents owns. Thus, we gain no further insights by using different measures for income or wealth.

Table 2.8: Logistic Regressions, Different Measures for Risk and Income

	Ann. Inc.	Dur. Assets	Risk	Invest
Male	0.097***	0.099***	0.099***	0.098***
	(0.032)	(0.033)	(0.033)	(0.033)
Age	−0.001	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Years of Schooling	0.004	0.003	0.005	0.004
	(0.005)	(0.005)	(0.006)	(0.005)
Ann. Cons. per Cap. (log)			−0.016	−0.016
			(0.026)	(0.026)
Risk Taking, MPL	−0.004*	−0.004**		
	(0.002)	(0.002)		
Ann. Income per Capita	−0.000			
	(0.000)			
Assets Value		0.000		
		(0.000)		
General Risk Taking			−0.003	
			(0.008)	
Hypothetical Investment				−0.000
				(0.000)
Observations	829	829	828	828
Baseline Predicted Probability	0.367	0.367	0.367	0.367
McFaddens R-squared	0.045	0.046	0.044	0.043

Dependent Var.: Decision to separate (=0) or couple (=1). Average marginal effects reported. SE clustered at district level. District dummies not reported.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The alternative risk measures we employ are self-reported, general risk taking on a scale from 0-10 (Dohmen et al., 2011) and a question in which respondents have to decide how much to invest in a business from a hypothetical lottery prize

of 100,000 THB, where the chance is 50% that the investment is doubled and 50% that it is halved. Both measures do not have a significant effect on coupling. Regardless, the multiple price list measure seems to be the more reliable measure given its more detailed scale[8] and that it resembles our game the most.

Clustering. We cluster at the district level following Cameron and Miller (2015) to cluster at least at the primary sampling unit. They emphasize that clustering on an even higher aggregated level may frequently more suitable. However, in our sample, there is no straightforward level above district. In that sense, our method is the most conservative way to cluster our sample. Nevertheless, we also run regressions clustered at subdistrict and village levels (see Table 2.9).

Table 2.9: Logistic Regressions, Different Levels for Clustering

	Subdistr.	Subdistr.	Village	Village
Male	0.098***	0.067	0.098***	0.067
	(0.034)	(0.045)	(0.035)	(0.045)
Age	−0.001	−0.001	−0.001	−0.001
	(0.001)	(0.002)	(0.001)	(0.002)
Years of Schooling	0.005	0.013*	0.005	0.013
	(0.005)	(0.007)	(0.005)	(0.008)
Ann. Consumption per Cap. (log)	−0.013	−0.032	−0.013	−0.032
	(0.022)	(0.030)	(0.025)	(0.030)
Risk Taking, MPL	−0.004*	−0.004	−0.004	−0.004
	(0.002)	(0.002)	(0.002)	(0.003)
Advice from Neighbor	0.075*	0.086	0.075*	0.086
	(0.046)	(0.055)	(0.044)	(0.057)
Not Comparing		−0.129**		−0.129**
		(0.048)		(0.053)
Trust in Village		0.059**		0.059**
		(0.025)		(0.029)
Observations	829	521	829	521
Baseline Predicted Probability	0.367	0.359	0.367	0.359
McFaddens R-squared	0.048	0.061	0.048	0.061

Dependent Var.: Decision to separate (=0) or couple (=1). Average marginal effects reported. District dummies not reported.

* p < 0.10, ** p < 0.05, *** p < 0.01

[8] In principle, the investment question allows for a more precise measurement. Nevertheless, respondents only made 22 unique choices, with more than 50% of the sample choosing 50,000.

Effects remain unchanged except for sex and receiving advice from neighbors, which lose their significance in the subsample. This is due to the fact that clustering at a too low level cannot account for correlated error terms within each district. Another point worth mentioning is that the number of district clusters is small and that large-sample assumptions might not hold. However, our main results are robust to re-estimating our regressions using wild cluster bootstrap (results available upon request).

Skills. As final check, we add indices potentially indicating the comprehension of the concept of *ex post* risk sharing; these indices represent the skills of numeracy and financial literacy (see Table 2.10). Numeracy is measured by letting respondents answer 6 standard math equations, which gives us an index taking values between 0 and 6. A higher value of numeracy is related to an increased probability of coupling the lotteries and, thus, seems to have a similar effect as education (although education is only significant in the small sample). This result is not driven by a potential multicollinearity problem between education and numeracy.

For financial literacy, two standard questions from the literature (Lusardi and Mitchell, 2008) and two additionally for this survey designed questions are used. We find a positive effect on separating, which would be in favor of *ex post* risk sharing. However, the coefficient is only significant in the small sample. In order to better understand potential drivers, we analyze the four items defining the financial literacy index separately. We see that the effect is purely driven by answering the question on inflation correctly, the other questions are not significant. Thus, this might be an artifact of the small sample.

Table 2.10: Logistic Regressions, Numeracy and Financial Literacy

	(1)	(2)	(3)
Male	0.095***	0.065*	0.066**
	(0.033)	(0.034)	(0.030)
Age	−0.001	−0.001	−0.001
	(0.001)	(0.002)	(0.002)
Years of Schooling	0.003	0.012*	0.013*
	(0.005)	(0.007)	(0.007)
Ann. Consumption per Cap. (log)	−0.014	−0.032	−0.029
	(0.025)	(0.032)	(0.031)
Risk Taking, MPL	−0.004*	−0.004	−0.004
	(0.002)	(0.003)	(0.003)
Advice from Neighbor	0.081**	0.084*	0.097**
	(0.041)	(0.048)	(0.047)
Not Comparing		−0.127***	−0.127***
		(0.046)	(0.046)
Trust in Village		0.057**	0.062**
		(0.025)	(0.027)
Numeracy	0.030***	0.013	0.016
	(0.010)	(0.015)	(0.016)
Financial Literacy	−0.011	−0.029*	
	(0.013)	(0.017)	
Inflation			−0.124**
			(0.052)
Interest Rates			0.007
			(0.038)
Expected Utility			0.018
			(0.040)
Loan Conditions			−0.023
			(0.037)
Observations	829	521	521
Baseline Predicted Probability	0.367	0.359	0.359
McFaddens R-squared	0.052	0.064	0.074

Dependent Var.: Decision to separate (=0) or couple (=1). Average marginal effects reported. SE clustered at district level. District dummies not reported.

* p < 0.10, ** p < 0.05, *** p < 0.01

2.8 Conclusion

Inequality aversion is a well-established concept in the behavioral economics literature, which is also gaining policy relevance due to increasing income inequality in most industrialized countries (Alvaredo et al., 2017). Thus, for sound and evidence-based policy making, it is important to have a better, empirically substantiated, understanding of individual inequality aversion. This is currently hampered by the lack of a simple measure for inequality aversion that can be integrated into large household surveys. Here we propose such a new and simple measure. Due to its simplicity, it is less informative than a measure specified by theoretical models, but has the advantages that (i) it requires no further assumptions on expectations and preferences and (ii) can be implemented at relatively little cost in empirical studies.

We term this new measure "coupled lotteries." It is built on a single decision of individuals; whether they want to separate or couple a predefined lottery with their peers. Here, coupling means that the lotteries of two individuals are perfectly positively correlated, i.e. both receive the same outcome. As expected payoffs are the same for both choices, the decision for coupled lotteries reveals inequality aversion.

We find, in our sample of 850 poor households from rural Thailand, that about 60% of respondents prefer to separate, whereas 40% prefer to couple. As theoretically expected, the latter choice is related to being more risk averse and having social status concerns. In contrast to previous studies, but in line with Bellemare et al. (2008), our results suggest that men are more inequality averse than women and that previous studies might include confounding factors. As a competing explanation of the findings, one may argue that those individuals who prefer to separate are aware of the possibility of *ex post* risk sharing. However, we examine several variables underlying such awareness and expectation, with none supporting this explanation. In addition to these results, we find that our measure is related to real-life choices. Household heads who are inequality averse are more likely to have formal insurance for their household and more likely to diversify the crops they cultivate, which can be interpreted as a kind of informal insurance.

Our supplemental lab evidence from Germany shows that coupled lotteries and dictator game decisions are not correlated, providing further evidence that dictator and charitable giving are not reliable measures for inequality aversion. Otherwise, we find similar correlations in our German student sample to those

we find in our rural Thai sample. Social connectivity seems to be an especially important correlate of inequality aversion in both samples.

Thus, we conclude that our method gives us a robust measure of inequality aversion within our two diverse samples. Future research is needed to show whether this finding has further external validity and holds with various parameterizations.

Chapter 3

The Effect of Social Comparison on Debt Taking: Experimental Evidence

We thank Anik Ashraf, Dirk Engelmann, Jana Friedrichsen, Lea Heursen, Raji Jayaraman, Lukas Menkhoff, Renke Schmacker, Helke Seitz, Roel van Veldhuizen, Georg Weizsäcker, and seminar participants in Berlin for helpful comments. The paper also profited from discussions with conference participants at IMEBESS 2019, Utrecht. Financial support by the Collaborative Research Center TRR 190 is gratefully acknowledged.

3.1 Introduction

The number of over-indebted households is increasing worldwide (IMF, 2017). In Germany, for example, 6.9 million households were regarded as over-indebted (Creditreform Wirtschaftsforschung, 2017) and 7.5% of German households had negative assets in 2017 (Deutsche Bundesbank, 2019). These high levels of debt cannot be rationalized by conventional economic theory and, in this regard, households' borrowing behavior is vastly understudied. There are many open questions about the determinants of debt taking (Zinman, 2015). Despite this, household debt is a wide ranging problem, as over-indebtedness can pose a serious risk to household well-being and the economy as a whole (IMF, 2017).

One promising, but still very small, line of research studies the effects of social comparison and peer effects on debt levels. Georgarakos et al. (2014) find that individuals who believe their social circle to have higher income than themselves are more likely to hold debt. Agarwal et al. (2019) show, using lottery winners in Canada as exogenous variation, that unequal incomes in neighborhoods can lead to financial distress. These studies find evidence for the effect of social comparison on debt. However, what remains unclear are the mechanisms behind these effects. In this paper, we aim to shed light onto these mechanisms by performing a lab experiment in which we are able to disentangle two different channels.

We use three different treatments to disentangle the following two possible drivers: *social image concerns*, concerns about private information about oneself that is revealed to others, and *peer information*, the information about others that is revealed to oneself. Bursztyn and Jensen (2017) clearly distinguish between these two kinds of peer effects. By separating these two mechanisms, we also contribute to literature on peer effects in general.

The three treatments vary the way participants make and communicate a consumption decision: participants are supposed to buy one pen out of a set of different quality pens with money they have previously earned in an IQ-quiz. The amount earned depends on how participants perform in comparison to the others in the session. All the money not spent is lost. Thus, assuming cognitive ability to be a socially desirable trait, the consumption decision can be indicative of being a "lower" or a "higher" type. In the control treatment, the decision which pen the individual participant buys is kept private. In contrast, in the public treatment, each participant must announce their decision publicly. The decision of which pen to buy is made in private before the public announcement, but participants know about the announcement before they decide. In the information treatment, the consumption decisions of participants who made their decision previously is

shown to the participants on screen before they make their own decision. This way, participants are informed about the decision of others without identities being revealed. In all treatments, participants have the possibility to take out a loan to buy a higher quality pen. They can later repay this loan by exerting sufficient effort in a real-effort task (the slider task by Gill and Prowse (2012)) or by using their participation fee. To estimate not only between treatment effects but also the possible shift in preferences within participants, we elicit a non-manipulated consumption preference prior to the experiment by conducting an online survey and compare it to the decision in the experiment. Furthermore, we elicit a comprehensive list of character traits and interact these traits with our treatments to investigate who is susceptible to social comparison.

We have two main expectations regarding the outcome of our experiment. First, loan take-up in the public treatment is higher than in the control treatment, since "lower" types try to hide their type by buying a more expensive pen as a way to signal high IQ. Second, take-up is also expected to be higher in the information treatment than in the control, because of a preference for conformity or because new information is received by learning about other participants choices. However, we expect this effect to be smaller than the social image effect in the public treatment, because it could potentially also move in the opposite direction. Related to these hypotheses, we generally expect persons who take a loan to work harder in the slider task.

Our results are very surprising. Participants in the public treatment do not take more loans than those in the control treatment, but are more likely to buy a pen of lower quality than they could actually afford. Two likely explanations for this finding are that (i) they do not want to be perceived as smarter than their peers, as it might not be socially desirable; and/or (ii) they do not want to be publicly blamed for making other participants worse off as their "success" in the experiment is directly linked to the "failure" of others. Thus, they still exhibit social image concerns but of a different kind than we hypothesized. We expected social image concerns to coincide with social status concerns in our experiment but clearly have to refute this expectation. In the information treatment, we find strong evidence for a taste for conformity. Lower performing participants do take out slightly more loans while higher performing participants buy a lower quality pen than participants in the control group. This means that participants in the information treatment converge to some average quality pen although adjustment from above is much larger than from below.

These results are supported when comparing pre-experiment preference to actual choice in the experiment: looking at the effects of our treatments within subjects, we can see that participants in the public treatment buy more lower quality pens, whilst participants in the information treatment adjust their choices to match those who have chosen before them, which is not the case for participants in the control treatment. Surprisingly, the amount of loan taken is negatively related to performance in the slider task for all treatments, even when controlling for general ability and motivation. This means participants do not want to make up for the lost earnings with extra work.

Beyond helping to broaden out knowledge about why private debt might have increased sharply worldwide, our study is linked to at least two other strands of the literature. First, to the literature on peer effects on consumption decisions in general and through which channels these effects actually might be induced. Second, on the discussion started by Veblen (1899) over 100 years ago on who is actually engaging in conspicuous consumption and is even willing to take out a loan to finance it.

Several studies find that social comparison at least influences consumption decisions. In the field, Kuhn et al. (2011) find that the likelihood of buying a new car increases if someone in the neighborhood has recently bought a new car (see also Grinblatt et al., 2008). Rural villagers make consumption decisions that are more in line with the decisions of those that they observe (Grohmann and Sakha, 2019). In the lab, people are less likely to take up additional assistance when this has to be done publicly to compensate for a smaller payout due to low cognitive ability (Friedrichsen et al., 2018). Methodologically, the experiment by Clingingsmith and Sheremeta (2017) is closely related to ours. They show in the lab that participants increase consumption of a "luxurious" good if income is linked to a desirable trait and the decision has to be made publicly. Our experimental design resembles theirs. However, we explicitly look at taking out a debt to consume and we have an additional treatment to test for peer information effects.

As previously noted, concerning debt taking and peer effects, we find only a few studies looking explicitly at this relationship. Bertrand and Morse (2016) describe, in their appendix, how the presence of higher top-income households is probably causally related to higher credit and bankruptcy in low-income households. As mentioned before, Georgarakos et al. (2014) find a link between beliefs on the affluence of the own social circle and debt taking. Agarwal et al. (2019) show that having a lottery winner in the neighborhood leads to increased rates of

bankruptcy for others in the area. While the former studies depict correlations, the latter does not discusses through which personal channels peer effects work and who is more responsive to social comparison.

Many of the aforementioned field studies implicitly explain higher "visible" consumption merely as a result of conspicuous consumption. In reality it is hard to disentangle whether persons want to convey status with their visible consumption or they just "learned" from the visible consumption of others and want to conform. There is a substantial literature on how social -peer- information affects pro-social behavior and charitable giving (e.g Frey and Meier, 2004; Shang and Croson, 2009; Smith et al., 2015) but less so on (debt-financed) consumption.

We see two major contributions of this lab experiment. First, we are able to study the mechanisms behind an effect that is found in a number of studies based on field experiments. Second, we test if effects, such as the social comparison effect that holds in studies on consumption, also hold once consumption can be financed through debt.

The paper proceeds as follows: Section 3.2 explains the experimental design and the data we collect. Section 3.3 reports our main results and Section 3.4 presents robustness checks. Section 3.5 discusses our findings in more detail while Section 3.6 concludes.

3.2 Experimental Design

Our experiment is designed to mimic consumption decisions in social situations when credit is available. The treatments vary in how the consumption decisions are made. There are three main stages to our experiment. A schematic description of the different stages is shown in Figure 3.1.

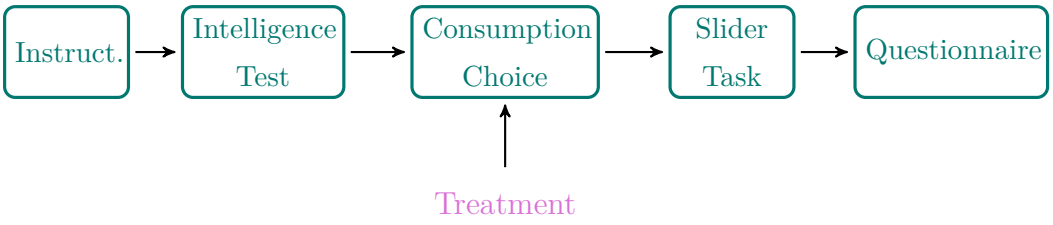


Figure 3.1: Experimental Flow

Once in the lab, participants first read the instructions and enter an individually constructed ID on the computer. Instructions are given in written form and on screen (see Appendix B.3.1). Then, the participants have to answer comprehension questions on the screen regarding the procedure of the experiment and

payoff possibilities (see Appendix B.3.2). If there are participants who have made mistakes in this part, the experiment only continues after one of the experimenters explains the right answer to these persons.

3.2.1 Intelligence Test

Following the instructions and comprehension questions, participants take an IQ-style test, for which they are paid according to their performance in relation to others in the session. All questions are taken off a website endorsed by Mensa and designed by a member of Mensa. Participants are made aware that these type of questions are used to measure intelligence. The questions are not designed to give a reliable IQ measure but present small intelligence riddles with varying levels of difficulty. We include questions for several levels of difficulty as defined by the website. Our test consists of twelve questions: four questions on completing number sequences and eight questions on completing sequences of pictures with geometric forms (see Appendix B.3.3). Participants have seven minutes to answer as many questions as possible. An intelligence test is chosen by us because we believe it is intrinsically desirable for our student sample to perform well on it. The top performing quartile in each session is paid 3€, the third is paid 2€, the second is paid 1€ and the bottom quartile is paid 50 Cents.

The test has an adequate level of difficulty. Out of the twelve questions the best candidates answered nine questions correctly and the worst none. The average candidate gave 4.43 correct answers and, with a standard deviation of 2.14, there is a good spread in the number of questions answered correctly.

3.2.2 Consumption Choice

The quiz is followed by a "shopping round," in which participants can buy a pen with the money they just earned. The pen is available in 5 different qualities and we use a star rating to convey the difference in quality: the five-star pen costs 4€, the four-star 3€, the three-star 2€, the two-star 1€, and the one-star pen costs 50 Cents. Thus, there is a pen quality for each earnings level and one pen whose price exceeds maximum quiz earnings. A picture of the pens and their labels is placed by each computer at the beginning of the session (see Appendix B.3.4). The lab prices present the actual list prices of the pens that are all from the same brand. The labeling makes it clear that the more expensive pens are supposed to be more desirable than the cheaper ones. To buy a pen that costs more than what participants earned during the IQ-test, participants can take a

loan of up to 3.50€. All the earnings from the quiz that are not spent on a pen are lost. The way the choice of a pen is communicated and what the participants know about the choice of others varies between treatments. For more details, see Subsection 3.2.3.

One of the reasons why we choose pens is that we assume that preferences are rather unidimensional in the sense that the price is the most decisive factor in the preference relation for the five pens, at baseline. This might be not the case for products like chocolate (see the Descriptives in Subsection 3.3.3). Simply speaking, for pens the price is more important than personal taste for color or material. Hence, there should be no other reason to buy a lower quality pen except for that it is cheaper. However, this means even in a standard economic framework without peer effects, incentives to buy a lower quality pen than one can afford are relatively small or non-existent in our experiment. This is the case because all the earnings from the IQ-quiz that are not spent on a pen are lost. Hence, participants would leave money on the table. However, as outlined before, in the absence of peer effects, there is also no particular incentive to buy a more expensive pen. Thus, pens are less likely to confound our treatment than other products, as the preference is more easily malleable.

3.2.3 Treatments

We assume that the most revealing signal for being in a particular earnings/performance group is to buy the pen whose price exactly corresponds to this group. That is because both pen prices and the earnings structure for the intelligence test are common knowledge:[1]

$$Pr_{-i}(\sigma_i = T | a_i = T) > Pr_{-i}(\sigma_i = T | a_i \neq T), \tag{3.1}$$

where σ_i is the type of individual i, $T \in \{1, 2, 3, 4\}$ is the type space, which in our case are the four possible performance groups and $a_i \in \{1, 2, 3, 4, 5\}$ is the action space, thus the 5 possible pens an individual can buy. $Pr_{-i}(\sigma_i | a_i)$ represents the probability that the other participants think individual i belongs to a certain group given a certain action. Furthermore, we assume that buying the most expensive pen makes it most likely for the other participants that i belongs to the highest (the fourth) performance group:

$$Pr_{-i}(\sigma_i = 4 | a_i = 5) > Pr_{-i}(\sigma_i \neq 4 | a_i = 5) \tag{3.2}$$

[1] Our notation is largely taken from Bursztyn and Jensen (2017).

With these assumptions in mind, we explain the treatments and their consequences in the following.

Private Treatment: Control
The private treatment is the control treatment. In the shopping round participants simultaneously decide on their individual computer screens which pen they want to buy. The decision is kept private and they continue to the slider task without any further intervention. In this treatment, social image concerns and peer information do not occur as there is no possibility for participants to infer which pen the others buy (which means $Pr_{-i}(\sigma_i|a_i)$ is not defined). The pens are handed over individually in another room next to the lab and participants leave after payout.

Public Treatment: Social Image Concerns
In the public treatment, participants again first make the decision simultaneously. However, after everyone made their decision they have to stand up one after another and have to publicly announce which pen they have chosen. The order in which participants stand up is random. They are informed beforehand that announcing the consumption decision is part of the procedure and are shown by the experimenter how they have to do it. Given our previous assumptions, buying a low quality pen is a strong signal for low performance in the IQ-quiz. Hence, further assuming that being seen as intelligent is a desirable trait, social image concerns can potentially occur, as $Pr_{-i}(\sigma_i|a_i)$ is well-defined. This is true especially for those individuals who end up in the lower performance groups. Thus, we assume that low performers in the public treatment are more likely to take a loan to buy a higher quality pen in order to signal higher intelligence.

Information Treatment: Peer Information
The third treatment is an information treatment, where each respective participant makes their decision sequentially, in random order, instead of simultaneously. Therefore, we can show participants in a small table on their screen how many pens of each kind have been bought previously (see Appendix B.3.6). Everyone is sitting in a cubicle with high walls and instructions are solely given on the computer without any interruptions from the experimenters. In this way, participants are informed about what their peers decided while no identities are revealed. This means that $Pr_{-i}(\sigma_i|a_i)$ is not defined because i cannot be identified, but that i has several a_{-i} that she can consider when making her own decision. Therefore,

peer information can occur but social image concerns are very unlikely to play a role. We expect to find that participants in this treatment will follow the decision of those who have already made their decision.

3.2.4 Slider Task

After the consumption choice, participants perform the slider task developed by Gill and Prowse (2012). The slider task is a computerized real-effort task where participants have to move a predefined number of "sliders" to a predefined position with their cursors (see Appendix B.3.5). In our experiment, they have four minutes to move up to 48 sliders to the value "50." Effort is measured by counting the number of correctly adjusted sliders. Moving the sliders is rather cumbersome and non-entertaining. Furthermore, we implement a sharply decreasing marginal return to effort: the first eight correctly adjusted sliders pay 25 cents each, the next eight earn 15 cents each, the following eight get 10 cents each, the next eight earn 5 cents each, the following eight 3 cents, and the final eight 2 cents. The slider task gives participants who previously took a loan the chance to earn additional money to repay that loan. After the slider task, final earnings from the experiment are calculated. If participants decide to take up a loan and do not exert enough effort in the slider task to repay it, the money is taken off the participation fee.

The average number of sliders set correctly is 23.83 and the maximum is 48 out of 48. This is in line with performances in other experiments that involve slider tasks such as Gill and Prowse (2019).

3.2.5 Pre-Experiment Survey

In their invitation email to the experiment, participants are asked to complete an online survey that was created with *Google Forms*. Invitation emails are send out one week before the sessions take place and participants are reminded to fill out the survey 1-2 days beforehand. In the survey, they have to provide an individual ID so that we can later link these data to the data collected in the experiment.[2]

In the online survey, participants are asked for their preferred product out of a group of five homogeneous goods. They have to indicate their favorite type of chocolate, cola, folder, lip balm, and pen. We show them a picture and the

[2] The ID is composed of the third letter of the first name + the last two numbers of the zip code + the last letter of the last name in capitals + the birthday for each individual participant. In this way, we can merge the online survey with the experimental data whilst participants remain anonymous and no sensitive data is collected by the researcher.

list price of each product (see Appendix B.3.7). The five pens are the same pens that they later can buy in the experiment. Thus, a pre-treatment preference for pens is elicited that we use for a within-subject analysis. By asking for a variety of homogeneous goods, we can reduce priming as participants are less likely to remember their choice. We further get an indication on whether the price is a decisive factor when choosing a pen and whether this is different for the other products. Additionally, we include questions on the importance of price, brands/image, and the opinion of others when buying small, everyday products like the products in the survey. These questions are measured on a Likert-scale from one to seven.

3.2.6 Individual Characteristics

We not only want to analyze the possible channels through which peer effects might increase debt taking but also who responds to which channel. Various studies look at differences in socially contingent consumption rather along socio-economic lines (like income, region, "race" etc.). We want to complement the literature by investigating what kind of personal attitudes and characteristics make persons more or less susceptible to social image concerns and responsive to peer information. We concentrate on five distinct personality concepts, namely cognitive reflection, locus of control, global self-esteem, self-monitoring, and the Big Five personality traits. Each of these are measured with well-established methods from the literature. *Cognitive Reflection* measures a specific type of intelligence: the tendency to reflect on problems rather than following a wrong intuition when looking for an answer. We use the three questions originally introduced by Frederick (2005). *Locus of Control* presents the perceived control over the own life. Here, we use the scale used in the German Socio-Economic Panel (Wagner et al., 2007), which itself is based on Rotter (1966). The "Rosenberg Self-esteem Scale" (see Rosenberg, 1979; Ferring and Filipp, 1996; von Collani and Herzberg, 2003) is employed to assess *Global Self-Esteem* (GSE). In contrast to specific self-esteem, GSE is an overall feeling of self-worth that is not attached to a particular situation. *Self-Monitoring* describes the willingness and/or ability of individuals to adapt their behavior to different social situations and is measured with the revised self-monitoring scale by Snyder (1974) (see Snyder and Gangestad, 1986; Graf, 2004). *The Big Five* are measured using the short version of the big five inventory "BFI-S" (John and Srivastava, 1999; Gerlitz and Schupp, 2005). For detailed hypotheses, as well as results on the relationship between

these personality traits and susceptibility to social comparison, see Appendix B.1.

Additionally, we collect socio-economic variables like sex and age as well as data on lab experience, financial literacy, and risk preference. The financial literacy scale is based on Lusardi and Mitchell (2008) and complemented by own designed questions. Risk preference is measured with the question on general risk taking by Dohmen et al. (2011).

3.2.7 Procedure and Participants

Our experiment took place at Technical University Berlin in November 2018. Including three pilot sessions, 27 experimental sessions were run. Treatments were randomized at the session level and each session lasted between 42 and 58 minutes.[3] On average, participants earned 14.33€, including a show-up fee of 5€ and a participation fee of 3.50€. In total, 305 students from various disciplines participated. All sessions had at least nine participants and most consisted of twelve participants. The experiment is programmed using z-Tree (Fischbacher, 2007) and participants are recruited from the subject pool of the Technical University laboratory via ORSEE (Greiner, 2015). The experiment is registered in the AEA RCT Registry, RCT ID: AEARCTR-0003597.[4]

In Table 3.1, we present the descriptive statistics for the whole sample and the control group as well as the differences between control group and the two treatments, respectively. For the main analysis, we exclude the 35 observations from our three pilot sessions, as we changed the experimental procedure substantially after the pilot. As can be seen, our treatments are gender-balanced, with the average participant around 23 years old, having studied for 3 semesters, a monthly income of ca. 690€, and already participated in at least one other experiment in the lab. There is also no difference in intelligence as proxied by absolute performance in the IQ-quiz and cognitive reflection between treatments. At a first glance, the number of imbalances seem to be particularly high in our experiment in comparison to other studies. However, given the sample size and the large number of variables we are looking at, this is actually not surprising. Furthermore, an F-test on joint orthogonality of all variables on the treatment cannot be rejected (p-value=0.14). We still control for the imbalanced variables in most of our specifications.

[3] There is a significant correlation between duration and treatments with mean duration of 46, 49, and 53 minutes, respectively. The difference can be explained by varieties in the procedure.

[4] See https://www.socialscienceregistry.org/trials/3597.

Table 3.1: Descriptive Statistics across Treatments

	Full Sample	Control	Control-Public	Control-Info
Male	0.48	0.48	−0.00	0.01
Age	22.86	22.57	−0.58	−0.29
Education	3.36	3.27	−0.20**	−0.06
Students	0.97	0.99	0.05*	0.02
Semester	3.68	3.50	−0.62	0.09
Student Job	0.28	0.27	−0.03	0.01
Mthl. Income	688.36	713.84	18.35	60.98
Risk Preference	5.34	5.81	0.85**	0.64*
Lab Experience	1.89	1.95	0.07	0.12
Know Someone	0.31	0.18	−0.22**	−0.17*
Persons in Session	11.33	11.67	0.46***	0.58***
Correct Control Questions	4.74	4.76	−0.01	0.08
Correct Quiz Questions	4.43	4.55	−0.00	0.37
Financial Literacy	4.59	4.70	0.27	0.07
Cognitive Reflection	1.91	2.01	0.19	0.12
Conscientiousness	−0.01	−0.12	−0.14	−0.20
Neuroticism	0.03	−0.06	−0.22	−0.07
Extraversion	−0.02	0.08	0.28*	0.04
Openess	−0.02	0.08	0.18	0.13
Agreeableness	−0.01	−0.13	−0.26*	−0.10
Self-Esteem	−0.01	0.19	0.37**	0.23*
Locus of Control	0.00	0.13	0.29*	0.10
Self-Monitoring	−0.02	−0.02	0.02	−0.03
Observations	270	93	182	181

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Participants in the public treatment have a higher level of education but are slightly less likely to study than participants in the control treatment. They are less risk-seeking and less extraverted but more agreeable and have a larger internal locus of control. We find differences between the control and information treatments for risk-seeking and self-esteem but not in the education domain. Finally, although there were significantly less participants per session in the public and information treatment (which is, however, exogenous to the participants), participants in these treatment are more likely to know another person in their session. Since this study analyses peer effects, endogeneity in the peer group size could seriously jeopardize identification. However, given that participants are not aware beforehand in which treatment they will end up and that we randomized

the order of treatments between daytime and weekdays, we do not have reason to believe that real-life peers were more likely to sort into one or another treatment.

3.3 Results

3.3.1 Descriptives

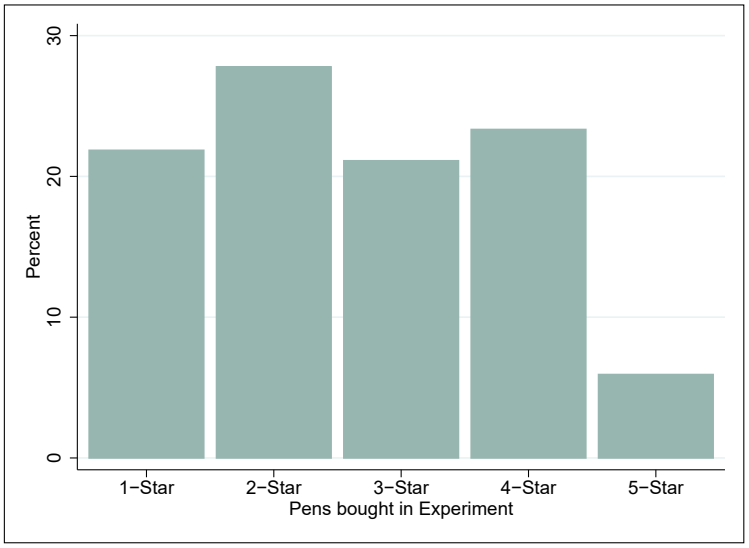
Overall, around 20% of the participants actually take a loan and the average loan amount conditional on take-up is about 1.30€. This means that, on average, participants take up a loan to buy a pen that is one quality level higher than the one they can afford with quiz earnings. However, as can be seen in Table 3.2, these numbers differ across treatments.

Table 3.2: Summary Statistics Outcome Variables

	Observations	Mean	Stand. Dev.	Minimum	Maximum
Control Treatment					
Loan Amount	93	0.22	0.62	0.00	3.50
Loan Dummy	93	0.17	0.38	0.00	1.00
Cond. Loan Amount	16	1.28	0.95	0.50	3.50
Public Treatment					
Loan Amount	89	0.23	0.53	0.00	2.50
Loan Dummy	89	0.21	0.41	0.00	1.00
Cond. Loan Amount	19	1.08	0.63	0.50	2.50
Info Treatment					
Loan Amount	88	0.31	0.73	0.00	3.50
Loan Dummy	88	0.20	0.41	0.00	1.00
Cond. Loan Amount	18	1.50	0.92	0.50	3.50

Summary statistics are given in Euro for *Loan Amount* and *Cond. Loan Amount*.

Figure 3.2: Distribution of Pens Bought in the Experiment



Similarly, in Figure 3.2, we see that participants borrow, with a considerable number buying the five-star pen, for which there is no corresponding earnings level. The two-star pen is the most frequently bought pen, whilst the three-star pen is the least popular. Most importantly, we can see from the graphic that people buy pens that do not correspond to their earnings level, as not all bars are of the same height.

3.3.2 Loan Take-Up

In Table 3.3, the effects of the socially contingent treatments on loan take-up are estimated.[5] For both treatments, there are no significant effects on whether participants took a loan nor on the amount, as seen in Columns (1) and (2). When controlling for imbalances, as seen in Column (3), effect sizes are much smaller for the loan amount in the public treatment, even negative, which suggests that participants in the public treatment actually take a smaller loan than those in the control group. Column (4) shows that there is absolutely no effect on the loan dummy when we control for imbalances.

Table 3.3: Effects of Treatments on Loan Take-Up

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Public Treatment	0.010	0.041	−0.017	0.006
	(0.118)	(0.088)	(0.137)	(0.095)
Info Treatment	0.086	0.033	0.073	0.003
	(0.126)	(0.078)	(0.114)	(0.073)
Mean Control Group	0.220	0.172	0.220	0.172
Controls	No	No	Yes	Yes
Observations	270	270	248	248

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Given the results on loan amounts for the control group and our sample size, we would be able to detect moderate effect sizes (Cohen's d = 0.37, assuming a power=80% and α=5%). This is almost exactly the same minimum detectable

[5] In all our regressions, we estimate standard errors that are bootstrapped and clustered at session level. However, given the subsequent small number of clusters, we also calculate p-values using wild cluster bootstrap following the advice of Cameron et al. (2008). All our results of interest are robust to this specification.

effect size we calculated in our pre-analysis plan and slightly larger than the effect found in comparable studies (for example Friedrichsen and Engelmann, 2018, find an effect of 0.3). However, the actual effect size of the public treatment is extremely small (Cohen's d = -0.017), the confidence intervals lie almost symmetrically around the null and never reach 0.3 in the positive direction. Hence, we are relatively confident that participants, in general, do not take a larger loan due to social image concerns.[6]

Effect sizes for the loan amount are larger in the information treatment, but standard errors are considerably high. However, observations in the information treatment might be path-dependent within each session and, in Table 3.3, we do not account for this. Table 3.4, tries to take this into account.

Table 3.4: Effects of Treatments on Loan Take-Up, Info Treatment Correction

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Info Treatment	−0.108	−0.173	−0.016	0.070
	(0.210)	(0.118)	(0.466)	(0.368)
Order	−0.017	−0.010*		
	(0.014)	(0.006)		
Interaction Order*Info	0.028	0.028**		
	(0.025)	(0.014)		
Mean Prev. Pens			0.023	0.051
			(0.114)	(0.116)
Interaction Mean*Info			0.034	−0.015
			(0.169)	(0.138)
Mean Control Group	0.220	0.172	0.220	0.172
Correction	Order	Order	Mean Pen	Mean Pen
Observations	248	248	226	226

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

* p < 0.10, ** p < 0.05, *** p < 0.01

In Columns (1) and (2), we control for the place in the order in which participants decide in the information treatment. Those who have to decide later

[6] There is the possibility that participants hide their low performance, but we still do not find an effect: they simply lie when announcing the decision publicly. Controlling for this possibility by cross-checking each announcement with the data, we do not find a single person who lied in the public treatment.

are more likely to adjust their behavior as they receive more information.[7] We find a significant order effect on the probability to take a loan at all, however, no significant effect on the amount taken. The effect size is small, which might be the case because early decision makers can either set a high or a low benchmark. If early decision makers choose low quality pens, there might be no reason for followers to take a loan. Therefore, in Columns (3) and (4), we control for the average of pens bought up to the point when the respective participant has to decide. Here, we find an insignificant, albeit positive, effect on loan take up and a negative effect on the likelihood. Eventually, there is some adjustment in the information treatment. Thus, in general, there seems to be an effect of the info treatment if controlling for path dependency but effects seem to be too small to reach significance.[8]

Summarized, when looking at between-subject effects, results are rather surprising. The public treatment has no effect on loan take up, if anything it seems that people are borrowing less in the public treatment. The information treatment seems to have larger effects. In the next subsection, we examine within-subject results to gain further insights. Results on how different personal characteristics interact with peer effects are in Appendix Tables B.1.1 and B.1.2.

3.3.3 Deviation from Pre-Experiment Choice

In this subsection, we compare pen choices in the pre-experiment survey to pen choices during the experiment. Hence, we can examine whether our treatments let participants choose to buy a different pen from the one they claimed to use in everyday life. Therefore, we compare the pen that participants actually buy in the experiment to the pen they buy and use most in everyday life as stated in the online survey. This is not a test between stated and revealed preference, as in the online survey we already ask explicitly for usage and not preference. More importantly, we expect a difference between the two pen choices, even for the control treatment because of the experimental design in general. In this sense, we are interested in whether the treatments changed the choice of the pen above and beyond the change already induced by the experimental setting. As argued in Subsection 3.2.3, participants have a large incentive to buy the pen that corresponds to their earnings level, especially if the price is the most important criterion for the choice of pens. The latter assumption seems to be

[7] Since in the other two treatments there is no order that matters for the decision, we use the subject number to order these observations in the various specifications.

[8] Both corrections have advantages and disadvantages. We prefer the order approach as it allows us to keep all observations, which is not the case if using the mean approach.

valid, especially compared to other goods, as seen in Appendix Figure B.2.1. In total, we collected 323 answers in our online survey and approximately 50% choose the cheapest pen. For all the other goods, no more than 24% ever choose the cheapest option. For example for folders, which belong to the same group of goods as pens (stationery), only 16% choose the cheapest.

Unfortunately, despite having more survey responses than participants, not all our participants answered the online survey or used different IDs such that we cannot merge their responses with the experimental data. We are able to match 219 cases that are evenly distributed between treatment groups (for each treatment we have about 80% who answered the online survey). Furthermore, there are no significant personal differences between those for whom we have valid answers and for those we do not (see Appendix Table B.2.1).

In Table 3.5, we regress the different pen choices on treatments. As expected, there are no significant effects on pre-experiment choices (Column (1)). However, there are also no significant effects on choices in the experiment (Column (2)). Interestingly, there is a change in signs, which means that there is a considerable difference between the two coefficients. This difference is marginally significant in the public treatment but only if we do not control for imbalances. Nevertheless, it seems that participants in the public treatment not only take a smaller loan but choose a cheaper pen in general.

Table 3.5: Pre-Experiment Choice and Adjustment

	Pen Before	Pen After	Difference
	(1)	(2)	(3)
Public Treatment	0.098	−0.061	−0.237
	(0.226)	(0.231)	(0.246)
Info Treatment	−0.227	0.022	0.262
	(0.254)	(0.179)	(0.289)
Mean Control Group	2.00	2.69	0.71
Observations	201	248	201

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Similar to Table 3.4, Table 3.6 shows the results for the info treatment, but controlling for order effects. Here, we find highly significant effects. Participants in the information treatment who decide later in the order buy a more expensive pen and, thereby, a pen that is further away from their pre-experimental choice.

Table 3.6: Pre-Experiment Choice and Adjustment, Info Treatment Correction

	Before	After	Difference
	(1)	(2)	(3)
Info Treatment	−0.101	−0.550*	−0.567
	(0.473)	(0.326)	(0.520)
Order	0.035	−0.064***	−0.102***
	(0.045)	(0.023)	(0.036)
Interaction Order*Info	−0.022	0.092***	0.134**
	(0.055)	(0.035)	(0.054)
Mean Control Group	2.00	2.69	0.71
Correction	Order	Order	Order
Observations	201	248	201

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

3.3.4 Leaving Money on the Table

So far, we have seen that participants in the information treatment are more likely to take a loan and, hence, more likely to buy a more expensive pen, if they have to make their choice later in the order. At the same time, we have seen that, in the public treatment, there is neither a significant effect on loan take up nor on the choice of pens compared to the choice in the pre-experiment survey. The surprising non-results in the latter treatment seem to not only be driven by small effect sizes in combination with a small sample, but effects seem to be non-existent or actually go in the opposite direction.

We here examine this further by looking at whether participants leave money on the table by buying a cheaper pen than the one they could afford according to their earnings. In Table 3.7, we determine if people leave money on the table and how much they leave. Results are striking, as participants in the public treatment buy significantly more lower quality pens and are more likely to do this than those in the control treatment. As expected, this effect is driven by high performers, which means we have an asymmetry: high performers are adjusting downwards but low performers do not adjust upwards.

Table 3.7: Buying a Lower Quality than Affordable

	Lost Amount (1)	Lost Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Public Treatment	0.115***	0.076**	−0.066	−0.033
	(0.043)	(0.030)	(0.073)	(0.044)
Info Treatment	0.068	0.067	−0.018	0.005
	(0.056)	(0.053)	(0.044)	(0.051)
Performance			0.002	0.002
			(0.003)	(0.003)
Interaction Rank*Public			0.026*	0.016**
			(0.015)	(0.007)
Interaction Rank*Info			0.013	0.009
			(0.009)	(0.008)
Mean Control Group	0.038	0.043	0.038	0.043
Observations	248	248	248	248

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

As in the previous subsections, we repeat these calculation for the info treatment by controlling for order effects. Results are shown in Table 3.8.

Table 3.8: Buying a Lower Quality than Affordable, Info Treatment Correction

	Lost Amount (1)	Lost Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Info Treatment	0.187**	0.183**	0.203**	0.233**
	(0.078)	(0.085)	(0.091)	(0.107)
Order	0.012*	0.012*	0.014*	0.013*
	(0.007)	(0.006)	(0.008)	(0.007)
Interaction Order*Info	−0.020**	−0.020*	−0.021*	−0.026**
	(0.010)	(0.010)	(0.011)	(0.012)
Mean Control Group	0.038	0.043	0.038	0.043
Controls	No	No	Yes	Yes
Observations	270	270	248	248

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

We can see here that people in the information treatment are also more likely to leave money on the table than people in the control group. However, in line with previous results, this effect is counteracted if participants make their choices later in the order. Thus, in contrast to the public treatment, there seems to be a rather symmetric adjustment in the information treatment.

3.3.5 Results on Effort Provision

As described above, the choice of pen is followed by the slider task. It is our expectation that participants who took a loan in the consumption stage will try to make up for their loss in income by exerting additional effort in the slider task. In Table 3.9, however, we find exactly the opposite: the amount of loan taken is significantly negatively related to effort. The treatments themselves seem to have no additional effect on the effect exerted in the slider task.

Table 3.9: Effort and Loan Take-Up

	Effort Slider Task (1)	Effort Slider Task (2)	Effort Slider Task (3)
Public Treatment	1.064	0.952	1.931
	(1.386)	(1.416)	(1.551)
Info Treatment	2.131	2.053	2.145
	(1.547)	(1.555)	(1.647)
Loan Amount	−3.397***	−3.254***	−2.023*
	(0.862)	(0.866)	(1.172)
Performance		0.348*	0.359*
		(0.190)	(0.191)
Interaction Loan*Public			−4.023**
			(1.574)
Interaction Loan*Info			−0.748
			(2.091)
Mean Control Group	22.61	22.61	22.61
Observations	248	248	248

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

In Column (2), we can see that there is a positive relationship between performing well in the IQ-quiz and performing well in the slider task. One can only speculate about the reasons behind this. It is possible that some people have a high general ability. Alternatively, low performers may have been demotivated

by their low performance and, as such, put little effort into the slider task. In Column (3), we see that the effect of having taken a loan on putting no effort into the slider task is largest in the public treatment. All the results combined indicate that having taken a loan in the consumption round may have demotivating effects later in the experiment.

Given the low loan take-up, the payoff from the slider task is too generous. Only two persons did not manage to work enough to repay their loans, all the others mostly obtained a surplus from the slider task. This makes it hard to draw meaningful conclusions, since participants did not have to work more to repay their debts.

3.4 Robustness

Controlling for the Pre-Experiment Choice There are slight, albeit not significant, imbalances across treatments in the pen participants have chosen in the online survey. Therefore, we control for this pre-experimental choice in Appendix Tables B.2.2 and B.2.3 and test if our main results are robust to this inclusion. Although our sample size is smaller, as not all participants answered the online survey, results regarding loan take-up in the two treatments stay the same. There is no significant positive effect of the public treatment on taking a loan. In this specification, coefficients are larger in size but all of them are negative. For the information treatment, we again find a significant and positive interaction between treatment and order of deciding. The effect is furthermore of a similar size than before. Interestingly, the more expensive the chosen pen in the online survey is, the larger is the loan amount in the lab. This indicates that participants did not give fun answers in the online survey, which is not incentivized, but reported truthfully.

Deviation from Pre-Experiment Choice - Dummy Given that participants deviate in both directions from their pre-experimental choice, we test if, in total, the treatments make it less or more likely to buy the pen that one actually prefers. In Appendix Table B.2.4, we find a small negative effect on the likelihood to buy the preferred pen, which is, however, not significant. If we control for order effects, the treatments seem to increase the likelihood to buy the preferred pen for first movers, but this effects fades out with the place in the order. Again, these effects are not significant as standard errors are extremely large. In general, the table supports our previous results as found in Tables 3.5 and 3.6.

Using a Different Order in Control and Public Treatment To correct for path dependency in the information treatment, we control for the order in which participants decide. However, since participants decide simultaneously in the control and public treatment, we have to use an artificial order for their choices. For our main results, we use the most straightforward order our data provide, which are the individual subject numbers that z-Tree is assigning to participants within each session. As a robustness check, we use a different ordering that is based on actual orders in the information treatment. For each potential number of total participants in the session, which are 9, 10, 11, or 12, we randomly draw one information treatment session and implement its ordering in the other two treatments. Results are presented in Appendix Table B.2.5. The interaction term between loan take-up and information treatment is almost the same in size and significance as the term in Table 3.4. The coefficients for leaving money on the table are smaller and not significant anymore. However, they still point in the same direction as before in Table 3.8 and their size is still large.

3.5 Discussion

We find some results in this paper that we did not hypothesize. Our two main findings regarding the debt taking and consumption choices are, first that participants buy worse quality pens than they can afford. This effect is weaker for participants in the info treatment who make their choice later in the order of participants. Secondly, and most strikingly, participants do not want to signal intelligence to other participants. Here, we discuss four potential reasons for the observed findings. These are "standing-out-aversion," "smarty-pants-effect," "blame aversion," and conformity.

"Standing-Out-Aversion" Jones and Linardi (2014) formulate a simple model and find evidence for what they call wallflowers: Some people are averse to being seen as too selfish or as too generous, they do not want to stand out with their level of generosity. Therefore, they adjust their action to what they believe the average is doing. If we directly translate this model from reputational to social image concerns and apply it to our experimental design, we should see that loan take-up is the highest in the public treatment. Given that the payout and performance structure is common knowledge, we assume that participants expect that the average person buys a two-star or three-star pen. Thus, low performing persons would have to take a loan to match the mean decision. This is not what we

find. We do find that high performing persons leave money on the table to buy a cheaper pen in the public treatment. However, this asymmetry does not support "standing out aversion" as an explanation. Further evidence against this explanation is that we find no differential effects for females and males (see Appendix Table B.2.6). Jones and Linardi (2014) find females are especially likely to be wallflowers and, if anything, our coefficients point in the exact opposite direction.[9]

"Smarty-Pants-Effect" Our participants avoid signaling higher intelligence by not taking a loan and buying a cheaper pen than they can afford. McManus and Rao (2015) find similar results to ours in a very different experiment. They present three explanations for this avoidance, of which two might be present in our setting. The first might be what they call "smarty-pants-effect," which means that participants neither want to appear smarter than their peers nor to be perceived as arrogant. This same effect is more prominently known as the "acting white" effect (e.g. Austen-Smith and Fryer, 2005; Bursztyn et al., 2019).

"Blame Aversion" An alternative explanation is what we call "blame aversion," which relates to social preferences. There is evidence that persons care about negative externalities of their own performance on others in cases where relative performance determines payout (e.g Bandiera et al., 2005). In our experiment, high performing participants are the reason why low performing participants can only afford a low quality pen. Thus, inequality is inevitable and self-esteem damage is done. However, it might be the case that high performers do not want to publicly take the blame for others being worse off and, therefore, pretend to be a low performer. Eventually, with both kinds of explanations, smarty-pants-effect and blame aversion, participants in the public treatment might have social image concerns, just not the ones we anticipated.

Conformity Looking at the coefficients for the information treatment and controlling for order effects, we find a significantly high and positive effect of buying a too cheap pen. This slowly goes down with the order of deciding. Additionally, the number of different modes in bought pens is smaller in the informa-

[9] Another consideration is that participants in our design are not exactly standing out when buying a low or high-quality pen as a quarter of participants is expected to do so given the payoff categories. Still, since we did not elicit beliefs about what participants think others will do, we do not know whether some persons might think that they would be the only one making extreme choices. However, in this case, even more participants in the public treatment should be willing to take a loan.

tion treatment than in the control treatment, albeit not significantly. Standard errors are large but the effect size is relatively large as well (see Appendix Table B.2.7). Since we also find some significant, though much smaller, effects for loan take-up, it seems that decisions in the information treatment are mildly converging to some lower midpoint. In contrast to the public treatment, however, participants cannot observe the individual behavior of others in this treatment. Hence, the last two paragraphs presented explanations for the observed behavior in the public treatment, however, not for the information treatment. A preference for conformity seems to explain the observed pattern in the information treatment fairly well. As conformity, we define the intrinsic preference to align consumption decisions to those of others without others even learning about this (see Goeree and Yariv, 2015). Alternative motives, like self-image concerns and pure information gathering, are unlikely in our setting as participants especially adjust from above and pens are everyday products. Overall, some participants are actually willing to incur cutbacks as either they have to take on debt or end up with a lower quality pen to conform.

3.6 Conclusion

The number of over-indebted households is increasing worldwide (IMF, 2017). Hence, it is increasingly important to understand the drivers behind this process. This paper contributes to the emerging literature on household borrowing behavior. It analyzes the effects of social comparison on debt taking, examining two potential channels. Here, we argue that social comparison is one of the reasons leading to increased debt taking, which in turn leads to overindebtedness.

It is our aim to disentangle two channels that underlie social comparison. Therefore, we take our research question to the lab, as it is difficult to do this outside the lab. We design two treatments through which we want to separately examine social image concerns and peer information. While the former relates to how an individual wants to be perceived by others, the latter relates to how an individual themselves perceives the decision of others. Few studies disentangle these two effects.

The possible biggest caveat of our study is that borrowing in the lab is highly artificial, since participants cannot leave the lab indebted. Still, we believe that our experimental design is different from standard spending decisions and that participants thought of the possible loan they could take as creating a temporary

debt. The fact that participants who took a loan did not work more means that they actually left the lab with less money than the others.

Our results on how social comparison might affect borrowing are quite surprising. Social image concerns lead to underspending in our setup, hence, the exact opposite of what we expected. Potential reasons are that participants do not want to be perceived as more intelligent, which is contrary to our expectation, or that the more successful do not want to be blamed for the failures of others. We acknowledge that these reasons are peculiar for our setting and might differ in other environments where, for example, status is not only defined by intelligence and no perfect correlation between success of one group and failure of another group exists. We find striking results on peer information. There is convincing evidence for an intrinsic inclination to conform, which leads less to more debt taking by individuals in the lower tail but more to underspending by those in the upper tail of the performance distribution.

Our findings highlight that not only is borrowing underresearched but also intrinsic motivations like conformity and their effects on consumption and borrowing. Conformity leads to "sub-optimal" decisions on both sides of the distribution in our experiment as participants deviate from their intrinsic preference elicited before the experiment took place. In real life, conformity might disadvantageously hurt the low income households. Especially in countries with high income inequality, like emerging markets, conforming to an average level of consumption might lead to severe financial distress. Research looking at how inequality in neighborhoods affects financial distress seem to confirm this concern. Furthermore, that the upper end of the distribution is adjusting more in our setting might be purely driven by the fact the decision only involves simple pens. It cannot be expected that the rich downward adjust their consumption when it comes to products where quality differences matter much more. Given the extensive research on status consumption in the last 120 years, future research should concentrate more on peer information effects on debt-financed consumption, similar to what is done in the domain of pro-social behavior.

Chapter 4

In Two Minds: Uncertainty Preferences Among Entrepreneurs

We thank Roy Kouwenberg, Lukas Menkhoff, and Georg Weizsäcker for helpful comments. We are grateful to Francesca Dalla Pozza and Victoria Robinson, who provided excellent research assistance. Views presented are those of the authors and not necessarily those of the European Bank of Reconstruction and Development.

4.1 Introduction

In weak labor markets, entrepreneurship may be perceived as a necessity rather than as an attractive outside option chosen by motivated individuals. Theoretically, one becomes an entrepreneur if the expected returns are greater than what can be earned from a wage job. Expected returns to entrepreneurship will, in turn, depend both on actual profitability as well as perception-based factors, such as competence and tolerance of uncertain outcomes (van Praag and Cramer, 2001). From an individual, financial (management) perspective, entrepreneurship often seems to not be an optimal decision (see Hall and Woodward, 2010). However, if jobs are scarce relative to labor supply, necessity will push more people into entrepreneurship, even those with relatively low levels of perceived competence, low tolerance for uncertain gambles, or both. Hence, there are entrepreneurs of opportunity, i.e. those who prefer to take the chance to open a business, and entrepreneurs of necessity, i.e. those who feel to have no other choice than becoming business owners.[1] Understanding how these different motivations shape entrepreneurial choices is crucial for identifying the potential scope for policy interventions to avoid business failure and support prosperity.

In this paper, we explore the relationship between uncertainty tolerance and perceived competence to manage strategic uncertainty among entrepreneurs of necessity compared to entrepreneurs of opportunity. Additionally, we compare their decisions to those from another "risk-taking" group: return migrants (returnees). We carry out our study with real entrepreneurs from two middle-income countries with historically weak labor markets, Albania and Kosovo.

Our contribution is fourfold. First, we study two under-explored factors that could explain the different willingness to enter the market between the two kind of entrepreneurs: attitudes towards uncertainty and feelings of competence to manage strategic uncertainty. As many decisions surrounding entrepreneurship involve uncertainty, ambiguity aversion might be an important determinant of opening a business (see Gutierrez et al., 2020). Meanwhile, beliefs about the own competence to manage uncertainty could also explain different entry motives. Perceived competence can change individuals' perception of uncertain events, thus changing the trade-off potential entrants face. Moreover, as Wu and Knott (2006) suggests, entrepreneurship may be more driven by willingness to accept uncertainty regarding one's own entrepreneurial ability than general tolerance towards uncertainty. Furthermore, there is evidence that opportunity entrepreneurs' busi-

[1] The distinction between necessity and opportunity entrepreneurs is drawn in several studies on entrepreneurship, for example, Koellinger and Thurik (2012) and Calderon et al. (2017).

nesses are more profitable than those of necessity entrepreneurs. Calderon et al. (2017) find that this gap cannot be explained by different levels of education and only partially by better management practices. Fossen and Büttner (2013) estimate that the returns to education are lower for necessity than for opportunity entrepreneurs. Differences in the willingness to take uncertain gambles could explain these gaps as well.

Second, we also concentrate on non-standard uncertainty preferences, e.g. ambiguity aversion, a-insensitivity, and source preference, as standard expected utility models fail to explain the general prevalence of entrepreneurship and evidence on whether entrepreneurs are more risk seeking than non-entrepreneurs is mixed (for an overview see Astebro et al., 2014). Third, one reason for this mixed evidence might be that there are other occupational risk takers in the comparison group that are not accounted for. Therefore, we isolate return migrants from the rest of the population and use them as an alternative comparison group. Lastly, not only is this research question new to the literature on entrepreneurship but our sample also is novel in the literature on ambiguity attitudes. Thus, we contribute to understanding the generalizability of findings on attitudes toward uncertainty from conventional samples.

For our study, we design a laboratory experiment that measures ambiguity aversion and a-insensitivity in a comprehensive way, as well as willingness to take different kinds of uncertain gambles. Both ambiguity parameters are measured with the "matching probabilities" approach by Dimmock et al. (2016). We measure a-insensitivity, which is a form of probability weighting induced by ambiguity, because it is found to be correlated to real life choices like stock market participation, whereas ambiguity aversion is not (Dimmock et al., 2016). The overweighting of rare events and the underweighting of frequent events, i.e. the perceived likelihood of an uncertain event, could similarly explain why some people see an opportunity in opening a business and others not.

Willingness to take uncertain gambles is simulated with a modified version of the market entry game of Camerer and Lovallo (1999). In our modified game, participants are presented with choice sets where they have to choose between a lottery, where probabilities are unknown, and an outside option. In half of our choice sets, participants face a kind of uncertainty where the outcome of the lottery is determined by an uncertain number of competitors. This is referred to as strategic uncertainty, as uncertainty is neither given by nature nor an unknown source, but is explicitly given by the actions of other individuals.

To study the importance of feelings of competence to manage strategic uncertainty, we randomize a message that either conveys a rather neutral (control) signal regarding the ability to judge the actions of other participants or conveys an affirmative signal that certifies the participant to have correctly judged the choices of other participants. Participants receive this message before the market entry game. For all subsequent choices, we hypothesize that necessity entrepreneurs are more responsive to treatment because opportunity entrepreneurs' level of perceived competence might be already high and, thus, less mutable. The treatment is based on two ideas: the first is the idea that familiarity decreases ambiguity aversion (e.g Heath and Tversky, 1991; Kilka and Weber, 2001; de Lara Resende and Wu, 2010) and the second is the possibility that entrepreneurship might be driven by a greater willingness to accept uncertainty regarding one's own entrepreneurial ability. Both ideas predict that boosting feelings of competence about the decisions of competitors should result in more willingness to tolerate strategic uncertainty. Additionally, inspired by a finding of Holm et al. (2013) that entrepreneurs are more tolerant of social risk (trusting other people), we analyze if the treatment makes them more willing to pass a risky decision to another person instead of deciding on their own.

Our results show that there are not strong differences in ambiguity aversion between necessity and opportunity entrepreneurs but that the latter are significantly more a-insensitive. This hints at the fact that opportunity entrepreneurs do not have a different ambiguity attitude *per se* but perceive non-strategic ambiguity differently than necessity entrepreneurs. Moreover, return migrants seem to be slightly more ambiguity seeking than entrepreneurs for small and moderate probabilities and are also significantly more a-insensitive than necessity entrepreneurs. Looking directly at the uncertain gambles in the market entry game, necessity entrepreneurs are the least willing to take non-strategic uncertain gambles at baseline. Surprisingly, we find no strong baseline differences for strategic uncertainty. As expected, necessity entrepreneurs are the most responsive group for the treatment. They experience the largest boost for taking uncertain gambles. However, the positive experience significantly increases the tolerance towards non-strategic but not towards strategic uncertainty, which is not what we have expected. In contrast, returnees seem to be the least responsive, which might be because the competence message is more salient to persons who in real life engage in risky decisions with competitors. The results provide evidence that training for entrepreneurs, where entrepreneurial skills are strengthened, might be especially useful for those who have the feeling they have started a business

because there was no other choice. Our results are robust to order effects and how sure participants are about their judgement pre-treatment.

Literature. As aforementioned, results on whether entrepreneurs are relatively risk loving are mixed and only a very few studies look at uncertainty attitudes. Additionally, most studies on entrepreneurial attitudes use student samples (e.g. Gutierrez et al., 2020). There are some exceptions using subjects with real entrepreneurial experience. Djankov et al. (2006), using survey data, find that entrepreneurs are more willing to accept a (well defined) risk-neutral gamble. Macko and Tyszka (2009) do an experiment with students, where some have entrepreneurial experience. They do not find any strong difference between entrepreneurial and non-entrepreneurial students with respect to general risk preferences, but they do find that entrepreneurial students are more risk seeking in a task that is framed as a business decision. Caliendo et al. (2009) conclude that only opportunity entrepreneurs are more risk seeking than the general population but not necessity entrepreneurs. Similar to us, Gutierrez et al. (2020) study whether overconfidence and ambiguity aversion explain market entry among students. They find that entry is caused by overconfidence but only if outcomes depend on ability and not on luck. Entering ability-based markets is also influenced by ambiguity seeking if the uncertainty is caused by the own ability rather than by nature, independent of the level of confidence. The closest study to ours is Holm et al. (2013). They run a large scale experiment on entrepreneurs' tolerance for strategic uncertainty, using a random sample of CEOs and a control group of non-CEOs in China. They find that entrepreneurs are more willing to enter multilateral competition, hence strategic uncertainty, and are more tolerant to uncertainty originating from trusting others. They do not differ from non-entrepreneurs with respect to their tolerance of non-strategic uncertainty. Our study is different from the latter two in so far that (i) we test if there are systematic differences between necessity and opportunity entrepreneurs; (ii) we analyze if inducing competence to manage strategic uncertainty has an effect; and (iii) we use a clearer elicitation method for ambiguity aversion and look at a-insensitivity.

In general, potential to grow but also income uncertainty is high in Albania and Kosovo, which makes them relevant study subjects. Entrepreneurship is vital for economic development because not only does it create value with new ideas and products, it also contributes to competitive dynamism. In the model of Iyigun and Owen (1998), a sufficient initial stock of both entrepreneurial and professional

human capital are important to avoid a development trap. As Cusolito et al. (2020, p.7) note, "Increasing innovation is a key regional priority in the Balkans region[...]." Working in the two countries makes the comparison between return migrants and entrepreneurs particularly salient. Both countries have experienced a large outflow of people in the last decades and intentions to migrate remain high (European Bank for Reconstruction and Development, 2018). While international migration is a source of remittances, it also deprives the local economy of risk-takers, who might otherwise fuel economic development at home. Jaeger et al. (2010) find that people who had migrated between regions in Germany have higher self-reported willingness to take risks. Similarly, Gibson and McKenzie (2011) find that even among high-skilled individuals from three Pacific countries, risk preference is still an important determinant of migration.[2] Understanding uncertainty preferences of different kinds of occupational risk-takers can support policies targeted at providing opportunities for these people to stay in the region. Thus, our study also contributes to this understanding and finds that returnees' uncertainty preferences are mostly unchanged by the treatment in comparison to entrepreneurs.

We are not aware of another study that looks at differences between necessity and opportunity entrepreneurs with respect to uncertainty tolerance and feelings of competence. Furthermore, by isolating return migrants and using them as a comparison group, we shed light on how entrepreneurs might be different from other risk-tolerant people in the population. It also allows us to test whether entrepreneurs have disproportionate tolerance for uncertainty due to competition, compared to general income uncertainty, and if their response to our treatment is, thus, also different from those of returnees.

Our paper proceeds as follows: Section 4.2 describes the experimental procedures, design, our hypotheses, and the sample. Section 4.3 shows the main results followed by Section 4.4 providing robustness checks. Finally, Section 4.5 concludes.

[2] Using a sub-sample of this study, Gibson et al. (2019) show that risk preferences of Tongan migrants do not change due to migration but remain stable.

4.2 Experimental Design and Procedures

4.2.1 Procedures

We conducted experimental sessions in six municipalities across Albania and four municipalities across Kosovo between April and May 2019. These are shown in Figure 4.1.[3]

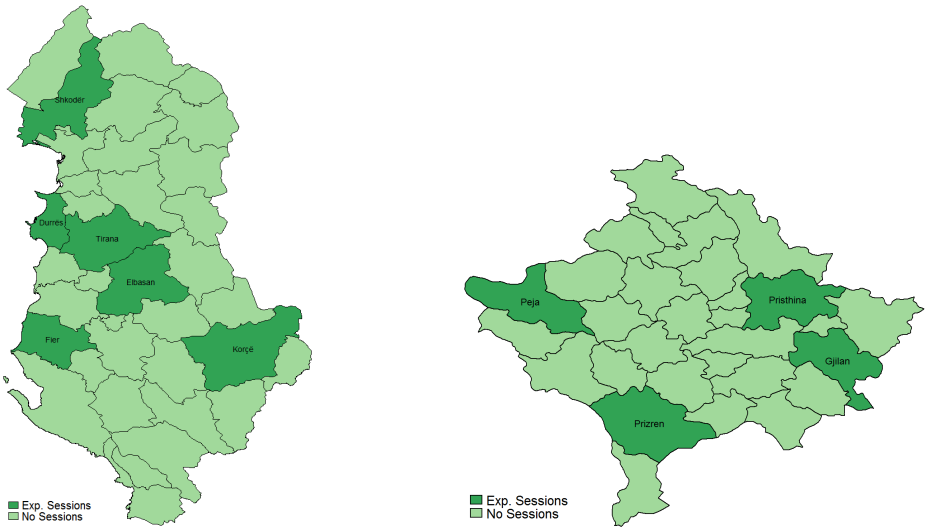


Figure 4.1: Cities in Albania (left) and Kosovo (right) where experiments were conducted

We ran a total of 21 sessions, with one, two, or three sessions per municipality.[4] The sessions took place in conference rooms in the center of the respective municipalities. In total, 224 persons participated, 121 in Albania and 103 in Kosovo. On average, they earned 19.30€ in Albania and 29.60€ in Kosovo for a session that typically lasted between 90 and 120 minutes.[5] Session were run in Albanian and all tasks were executed on tablets using oTree (Chen et al., 2016).

Our between subject experiment consists of five parts. Parts I-III occur before and parts IV-V after the treatment. In parts I and II, risk and ambiguity aversion parameters are measured. Part III looks at risk and uncertainty preferences with strategic uncertainty, pre-treatment. The treatment is administered between part

[3] The municipalities are Durrës, Elbasan, Fier, Korçë, Skhodër, and Tirana in Albania and Gjilan, Peja, Pristhina, and Prizren in Kosovo.

[4] Two municipalities had only one session each. Others included either two or three, depending on our success recruiting participants.

[5] Since average incomes and living standards differ across the two countries, we adjusted experimental earnings accordingly. Average earnings roughly equal a day's average wage in each respective country.

III and IV. Part IV assesses how different kinds of uncertain gambles are affected by the treatment. Part V includes a dictator, a trust game, and a short survey to collect demographic data. In parts I-IV, tasks are structured as choice sets, mostly with a varying number of multiple price lists. In many parts, price lists are not directly given but determined with a bisection method (see 4.2.2 for example).

All subjects were supposed to complete all parts of the experiment in the same order. This is because the experiment and associated tasks were novel to all the subjects, so all instructions had to be read out loud by a moderator and examples had to be demonstrated.[6] Subjects were paid in cash and for a randomly selected decision they made in each choice set. This was to eliminate confusion and ensure salience of each decision. The currency in the experiment were points that were later translated to money. In addition to experimental earnings, all participants received a participation fee of 1000 Lek (ca. 8€) in Albania and 15€ in Kosovo. The moderator handed out the payments at the end of the session in private. Participants remained seated until their name was called. The moderator showed them the amount they earned and the participant signed a receipt that they had received the money. Each participant left the room immediately after receiving their payment.

4.2.2 Measuring Ambiguity Aversion and A-Insensitivity

We elicit ambiguity preferences using an extension of the original Ellsberg experiment that allows us to calculate a more granular measure of ambiguity aversion and a measure for a-insensitivity. The measures are described below.

The Ellsberg Experiment

To cross-check the consistency of the extended method, we first replicate the original two-color Ellsberg problem. Participants are asked to choose between a risky bucket, which comprises 50 blue and 50 orange balls, and an ambiguous bucket, which also contains blue and orange balls but in unknown composition (see Figure 4.2).[7] A ball is randomly drawn from the chosen bucket to determine participants' payout from that choice.

[6] In pilot sessions, we read only general instructions out loud and randomized the order of decision tasks. This requires subjects to read some instructions independently. We discovered that there was a substantial risk that subjects would not fully understand all the tasks if left to read the instructions on their own. This was driven by subjects who were older, had less computer literacy, or had lower levels of education.

[7] For the same reason as Dimmock et al. (2016), we do not use black and red, the original colors, to avoid confusion for color-blind people.

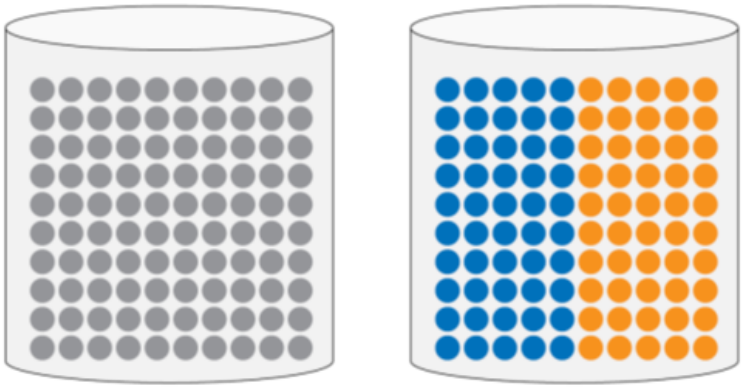


Figure 4.2: Ambiguous and Risky Bucket with Two Colors

Each subject makes two such choices, with the values of orange and blue balls being different in each choice. For the first choice, the subject earns 100 points if a blue ball is drawn and nothing if an orange ball is drawn. For the second decision, orange is the "winning color" worth 100 points and blue is worth 0 points.

First described by Ellsberg (1961) and confirmed by many studies afterwards, a substantial share of individuals chooses the ball to be drawn from the risky bucket in both choices, irrespective of the winning color. This preference violates not just expected utility theory as formulated by von Neumann and Morgenstern, but also classical models of subjective probabilities à la Savage (1954). Based on this, participants can be grouped in three categories: those who always choose the risky bucket are defined as *ambiguity averse*, those who always choose the ambiguous bucket are *ambiguity seeking*, and those who change buckets for different winning colors are considered to be *ambiguity neutral*.

Matching Probabilities

For our analysis, we concentrate on an elicitation method for ambiguity aversion that is an extension of the original Ellsberg problem. The method was introduced by Dimmock et al. (2016) and is called "matching probabilities." It allows us to generate more granular measures of ambiguity aversion. A matching probability is interpreted as the subjective probability of winning that is attached to the ambiguous bucket.

For the matching probability elicitation tasks, subjects make nine choices, split into three sets of three. In each set, subjects first chose between a risky bucket, with an objective chance of winning, and an ambiguous bucket, with an unknown chance of winning. In the subsequent two decisions in each set, we change the composition of balls in the risky bucket so that the risky bucket becomes less and

less attractive the more often it is chosen and vice versa. We fix the contents of the ambiguous bucket across the choices.

Using a bisection method, we identify the probability that makes the participant indifferent between betting on the risky bucket and betting on the ambiguous bucket. For example, assume that blue is the winning color and the chance of winning in the risky bucket is 50-50 in the beginning. If the participant chooses the risky bucket in the first round, she is presented with the choice between a risky bucket that now contains 25 blue balls and 75 orange balls and the ambiguous bucket in the second round. Hence, the objective probability of winning in the risky bucket decreased. If she had chosen the ambiguous bucket in round one, she would be presented the same ambiguous bucket but a risky bucket containing 75 blue balls and 25 orange balls. This bisecting continues for two further rounds. For simplicity, we fix the winning color for all participants to be blue.[8]

In the first choice set, each risky bucket has orange and blue balls like in the example above. The exercise is repeated for a second choice set, but instead of orange and blue balls in the risky bucket, the non-blue balls are of many colors. The risky bucket comprises balls of ten different colors with ten balls per color at the beginning (see Figure 4.3).

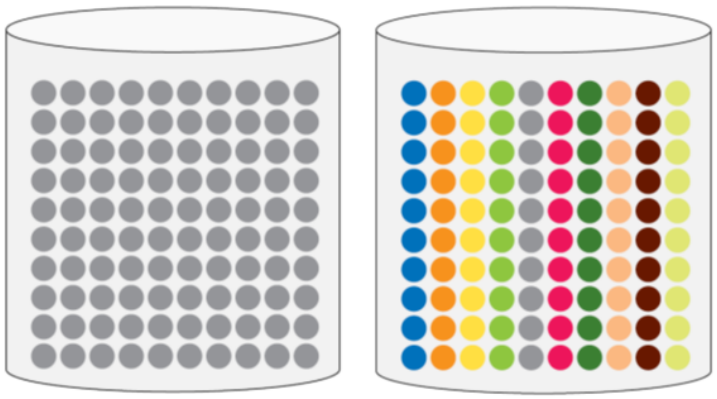


Figure 4.3: Ambiguous and Risky Bucket with Ten Colors

The participant wins 100 points if the drawn ball is blue and nothing otherwise, which means the objective probability of winning is 10% at the beginning. As before, the subject makes three decisions and we are bisecting depending on the previous choice. In a third set of choices, the same risky bucket as in set two is used. However, this time participants win 100 points if the drawn ball is NOT

[8] Furthermore, we do not include an "indifferent" button like Dimmock et al. (2016) did. Given the setting of the experimental session, we were afraid that the indifference button would invite participants to not consider their choices carefully, instead simply hitting the indifference button all the time.

blue, hence, the initial winning probability is 90%. Instructions and pictures accompanying the tasks are found in the Appendix C.4.2.

A matching probability is calculated for each choice set. We define the matching probability as the number of winning balls that would make the participant indifferent between betting on the risky and betting on the ambiguous bucket. We approximate this with the average number of winning balls in the risky bucket in the last choice and the number of winning balls if we had to bisect again in an additional fourth choice. This means that there are eight possible values the matching probability can take, ranging between 6% and 94% for the 50-50 bucket for example. We calculate three different matching probabilities for the objective winning probabilities of 10%, 50%, and 90%. Thus, we repeat the aforementioned exercise three times. Using the three matching probabilities, we are able to calculate local ambiguity aversion parameters by comparing the matching probabilities to their objective, ambiguity-neutral probability p. Dimmock et al. (2016) call these local ambiguity attitudes "event-specific indexes",

$$AA_{0.1} = 0.1 - m(0.1) \tag{4.1}$$

$$AA_{0.5} = 0.5 - m(0.5) \tag{4.2}$$

$$AA_{0.9} = 0.9 - m(0.9) \tag{4.3}$$

where $m(0.1)$, $m(0.5)$, and $m(0.9)$ are the respective matching probabilities. For all indices, a positive value means ambiguity aversion whereas a negative value means ambiguity-seeking. Furthermore, by fitting a linear function between the three p's and $m(p)$'s,

$$m(p) = c + sp \tag{4.4}$$

where c is the intercept and s the slope, we can generate an index for *global ambiguity aversion*, which is defined as:

$$b = 1 - s - 2c \tag{4.5}$$

Index b is the difference between the distance of the linear approximation from the value 1 at $p = 1$ and the intercept. If it is larger (smaller) than zero, the individual is considered to be ambiguity-averse (-seeking).

A-Insensitivity

Another kind of ambiguity-induced preference is *a(mbiguity-generated likelihood)-insensitivity*. A-insensitivity describes the feature that some individuals tend to anchor small and high probabilities to the mid-point, which means they cannot sufficiently discriminate between intermediate probabilities. It can be considered as a special form of probability weighting induced by ambiguity. With our approach, a-insensitivity can be calculated using two different procedures. First, simply by the difference between $AA_{0.9}$ and $AA_{0.1}$. An individual is a-insensitive, if $AA_{0.9}$ is positive (thus, the matching probability for 0.9 is smaller than 0.9) and if $AA_{0.1}$ is negative at the same time (thus, the matching probability for 0.1 is larger than 0.1). This means that a positive difference implies a-insensitivity and a negative difference a-oversensitivity. Second, we can generate the so-called index a from the linear approximation as:

$$a = 1 - s \tag{4.6}$$

In comparison to index b, index a solely relates to the steepness of the curve and captures the insensitivity to the likelihood of events that are not 50-50. In our experiment, both methods to derive a-insensitivity lead virtually to the same results. However, we will use the second derivation throughout our analysis, as this approach has a clearer decision-theoretic foundation. For a general, decision-theoretic foundation of matching probabilities, we refer to Dimmock et al. (2016). Essentially, matching probabilities are founded in the source method of Abdellaoui et al. (2011) and the axioms of Chew and Sagi (2008), which both follow the idea that subjective probabilities depend on the source of uncertainty.

Thus, we use the three matching probabilities, their values for local ambiguity aversion as well as indices b and a to assess ambiguity aversion and a-insensitivity in our analysis.

4.2.3 Competence Treatment and Choices under Uncertainty

The Treatment

After part two, where ambiguity aversion parameters are measured, participants face several choice problems in part three, in which they have to choose between two options. These options either entail a certain payout, a risky payout, or

a payout with strategic uncertainty. Recall that strategic uncertainty refers to uncertainty that is generated due to the actions of other individuals. To mimic strategic uncertainty, we use a "market entry" game (e.g. Camerer and Lovallo, 1999), in which the final payout depends on how many people choose to enter the market. The option is not referred to as market but as "competition" to make the competitive aspect of this game more salient. Entering the competition entitles participants to a share of a limited amount of profits. Furthermore, in our case, participants do not decide simultaneously to enter or not, but every participant is quasi-randomly matched to a group of four other persons who have done similar choice tasks in our pilot sessions before. These other persons are always either "competitors" or non-competitors, depending on how often they have chosen to enter competition. Competitors always enter the competition if the participant chooses this option, non-competitors never. This means that the number of other entrants is unknown but fixed. There is also no excess entry; the total number of winning points is divided by the number of all entrants, which means that participants gain a strictly positive amount. For example, if a participant has three other competitors in her group and she decides to compete over 300 points, she gets 75 points. The number of competitors is only important for payoff if the participant chooses the competition option. Participants are not informed about the number of competitors in their groups beforehand. Thus, the competition option entails strategic uncertainty.

Before participants start the tasks in part three, they have to guess the number of their competitors and how certain they are about their guess (see Appendix C.4.3). The only information they have is that given the their group size they can have at most four competitors. After they finish part three's choice sets, it is revealed how many competitors the participants actually have faced. Along with this revelation, we implement a "competence treatment" by matching the exact number of competitors guessed to half of the participants and a different number to the other half in each session. The following messages are shown to the participants:

Message Competence:
"Now that Part III is complete, you can learn how many competitors you faced. You guessed that you would have # competitors. You had #. Only half of the people in the room today had the same number of competitors as their estimate. Well done! You got mastery in the estimation task."

Message Neutral:
"Now that Part III is complete, you can learn how many competitors you faced. You guessed that you would have # competitors. You had ##. Half of the people in the room today had guesses that matched reality."

Where # is a number between 0 and 4 that equals the guess of the participant and ## is a different number between 0 and 4. Those individuals who are randomized in the neutral message make up the control group.

The "competence" message is supposed to exogenously strengthen the perceived confidence of our participants with regard to estimating the actions of other persons. Specifically, with this message two different kinds of confidence might increase: confidence related to estimating the absolute value of competitors correctly or confidence related to having better judgement than half of the other participants. Since participants have guessed their competitors correctly this time, they might feel confident in guessing it correctly again in other choices that entail strategic uncertainty. The reasoning behind this conjecture is that there is evidence that at least ambiguity aversion in the gain domain decreases with familiarity and knowledge about the source of uncertainty (see Heath and Tversky, 1991; Kilka and Weber, 2001; de Lara Resende and Wu, 2010). This might also be true for the tolerance of uncertainty in general. We analyze what kind of choices might alter with an increase in this kind of competence. Specifically, we are interested whether necessity and opportunity entrepreneurs are not only potentially different with respect to ambiguity aversion and a-insensitivity but whether they also respond differently to the treatment (and if both respond differently than other parts of the population). Wu and Knott (2006) conclude that entrepreneurs are not less uncertainty averse than the general population *per se* but the difference depends on the domain. Entrepreneurs seem to be more willing to accept uncertainty in personal ability, both in absolute terms and in relation to others. Entrepreneurs who have the feeling they have no other choice but to become self-employed might feel different than opportunity entrepreneurs in this respect and respond differently to the treatment.

Uncertainty Choices

After the treatment, four different choice sets are played in part four of the experiment. The first three sets make use of non-strategic uncertainty in form of the ambiguous bucket, introduced in part two of the experiment, strategic uncertainty as introduced in part three and a certain payment. One choice set

elicits the certainty equivalent (CE) for an ambiguous bucket that pays out 300 points if a blue ball is drawn by varying the certain payoff. Another set elicits the certainty equivalent for strategic uncertainty, where participants are again matched with four other persons in a market entry game. It is clearly explained that these four persons are not the same people as in part three. Hence, it is not clear to the participants how many competitors are in their group. The total profit to be shared among all competitors is 300 points, which means that participants get at least 60 points and at most 300. In this choice set, the certain payoff is again varied. In the last of the three sets, strategic uncertainty is held against non-strategic uncertainty. However, the total profit to be shared with competitors starts with 750 points and will change while the ambiguous bucket is fixed again.[9] For each choice set in this part of the experiment, there are four choices, where the values of the varying choice is again determined via bisection. The order in which the three sets are considered is randomly assigned to each participant. Instructions are found in Appendix C.4.4.

The fourth choice is slightly detached from the other sets since it measures a completely different aspect of decision making under uncertainty. It relates to taking or passing the responsibility for a risky choice to another person. Participants face a lottery where there is a 50% chance of winning 150 points and 50% chance of getting nothing. The lottery is determined by a bucket that contains 10 balls, which are numbered from 1-10. It can be decided if the 150 points are won if a ball with the number between 1-5 is drawn (which means that the participant receives nothing if the ball drawn has a number between 6-10), or if the 150 points are won if the drawn ball has a number between 6-10 (see Figure 4.4). Thus, participants cannot change the risk of losing but can decide the winning numbers. This is made clear to all participants upfront.

	Scenario A		**Scenario B**	
	150 points	if ①②③④⑤	150 points	if ⑥⑦⑧⑨⑩
	0 points	if ⑥⑦⑧⑨⑩	0 points	if ①②③④⑤ .

Figure 4.4: Lottery for Choice 4

For this choice, participants are matched with another, unknown, participant in the room. They receive the option not to choose the winning numbers them-

[9] The profit is chosen so high in this choice set to accommodate the most pessimistic belief that everyone enters the competition. In this case, to receive 300 points, which is the highest possible gain for the ambiguous bucket, the total profit has to be 1500 points.

selves but to hand over this task to their matched partner. The partner faces the exact same decision. Both are informed about the chosen winning numbers for both of them after they decide (see Appendix C.4.5). Although the partner cannot change the risk, participants might still feel unconformable to lay their fate in other peoples' hands. Bohnet et al. (2008) coin this feeling "betrayal aversion," which describes the greater unease of taking risks if the source is not nature but another person. At the same time, some persons might not want to take responsibility for their outcomes and, therefore, prefer to let the partner decide. In their study, Holm et al. (2013) find that entrepreneurs are more willing to take social risk, meaning more willing to trust other persons, than non-entrepreneurs. In the following Section 4.2.3, we discuss how our treatment potentially changes this preference.

Hypotheses

At outlined in Section 4.2.3, our treatment is supposed to increase the perceived competence, the ability to estimate the "competitiveness" of other persons. For each of the four choices above, we first formulate a hypothesis about if and how they are affected by the treatment. Then, we discuss how the two groups of entrepreneurs might differ from each other and other population groups in their response.

Feeling competent is domain dependent and the treatment should affect the confidence to judge other persons more than it affects confidence in judging nature. Since the composition of the ambiguous bucket is not related to the choices of other participants, we do not expect to find an effect on the certainty equivalent for non-strategic uncertainty.

Hypothesis 1: The treatment does not affect the certainty equivalent for non-strategic uncertainty.

In contrast to the ambiguous bucket's CE, the certainty equivalent for strategic uncertainty should be affected by the treatment. This CE is dependent on the belief of the participant about the number of competitors and how certain she is about her belief. Participants in the control group should perceive the prospect as less certain than those in the competence treatment and, therefore, rather bet on the certain amount. Their level of aversion to strategic uncertainty should be

higher independent of whether they expect to have a small or a high number of competitors, which means their CE is lower.

Hypothesis 2: The treatment has a positive effect on the amount of the certainty equivalent for strategic uncertainty.

As described in 4.2.3, ambiguity aversion reduces with experience, which might translate to the willingness to take uncertain gambles in general. Combined with the reasoning for hypothesis two, the treatment should increase participants preference for strategic over non-strategic uncertainty.

Hypothesis 3: The treatment has a negative effect on the potential total profit to be shared with other competitors, which makes participants indifferent between strategic and non-strategic uncertainty.

Judging other persons' market entry decision correctly might induce a feeling of "being an able entrepreneur" in the whole sample. Since entrepreneurs seem to trust other people more, larger perceived competence might increase the willingness to pass the responsibility to others.

Hypothesis 4: The treatment increases the probability that participants let others choose the winning numbers instead of deciding on their own.

Entrepreneurs out of necessity are expected to feel less competent than opportunity entrepreneurs at baseline because opportunity entrepreneurs would not have opened a business if they did not believe themselves to have some form of general ability with regard to entrepreneurial skills. Their level of perceived competence might already be high and, therefore, less immutable. Hence, we hypothesize that necessity entrepreneurs are more responsive to the treatment.

Hypothesis 5: Necessity entrepreneurs respond more strongly to the treatment than opportunity entrepreneurs.

Finally, we also formulate a hypothesis regarding how entrepreneurs and return migrants differ. As outlined before, both groups face considerable uncertainty in their "occupational choice." However, it should be expected that entrepreneurs are much more affected by the treatment as for them assessing the actions of other

persons is much more salient. The outcome of a business is directly dependent on the actions of other businesses in the market and potential entrants.

Hypothesis 6: In general, entrepreneurs respond more strongly to the treatment than do return migrants.

4.2.4 The Sample

Participants were recruited in two different ways: Initially, we asked respondents of a survey implemented by the European Bank for Reconstruction and Development (EBRD) in 2018 if they are interested in participating in an interactive session that would be carried out approximately one year after the survey and for which they will be compensated. The survey was set up to explore "recent trends in economic migration from the Western Balkans to Western Europe" and to analyze the link between migration and entrepreneurship. The survey respondents were drawn from screener samples that are representative for Albania and Kosovo. However, for the survey, potential migrants and those with a recent migration history were deliberately over-sampled. Nevertheless, respondents were still randomly drawn from the respective strata. In total, 2,301 persons in Albania and 2,323 persons in Kosovo participated in the survey. Out of these, 70 persons in Albania and 44 persons in Kosovo attended our experimental sessions. Additionally, we asked each survey participant to refer a friend or another family member who they thought would be willing to participate in an interactive session. Thus, the remaining experimental sample consists of these family members and friends.

For the analysis, we mostly concentrate on participants who either have entrepreneurial spirit or who have a migration history. Initially, we define entrepreneurs and returnees by the following means: *entrepreneurs* are all participants who set up a business successfully at least once but who do not have any migration history. We distinguish between entrepreneurs out of necessity and those out of opportunity by asking for the most important reason they opened their last business.[10] *Returnees* are defined as persons who have lived at least three consecutive months abroad in the last six years for non-recreational purposes but who do not have any entrepreneurial history. Naturally, there are

[10] The reasons provided for necessity are: 1. could not find (a suitable) job; 2. afraid of losing job at that time; 3. needed to earn more money; and 4. other. For opportunity: 1. ideal form of work; 2. opportunity to be in charge; 3. opportunity to earn more money; and 4. other. Our classification approach is similar to the one of the Global Entrepreneurship Monitor (see Reynolds et al., 2005).

persons in the sample who have both an entrepreneurial and migration history. Our analysis shows that these persons are much more like entrepreneurs and, therefore, they are included in the entrepreneurial group.[11] We group all other persons in a final group, which we call the *remaining* sample throughout the analysis. Table 4.1 shows descriptive statistics for entrepreneurs (necessity and opportunity), returnees, and the remaining sample.[12] A detailed explanation of the variables is in Appendix C.3.

Table 4.1: Descriptives of the Sample and Groups of Interest

	Entrepreneurs			Return	Remain
	All	Nec.	Opp.		
Male	0.55	0.68	0.44	0.68	0.43
Age	35.97	36.82	35.23	35.33	32.18
Education	6.07	5.53	6.54	4.47	5.57
Dummy Working	0.68	0.68	0.69	0.40	0.44
Work Status	2.62	2.43	2.78	2.50	2.00
Agreeableness	2.78	2.88	2.69	2.48	3.17
Extraversion	−0.14	−0.38	0.08	−0.85	−0.89
Conscientiousness	3.70	3.56	3.82	2.78	3.96
Neuroticism	−1.71	−1.24	−2.13	−0.83	−1.52
Openness	1.85	2.00	1.72	1.52	1.55
Observations	73	34	39	60	89

Groups: *All* are all entrepreneurs, *Nec.* and *Opp.* only include entrepreneurs out of necessity and opportunity, respectively, *Return* represents the group of return migrants and *Remain* all persons who are neither entrepreneurs nor returnees. Variables: *Male* is an indicator for being male or female; *Age* is the age of the participant in years; *Education* is a categorical variable from 1-9, where 1 is "no degree/no education" and 9 is "doctoral degree or equivalent;" *Dummy Working* is an indicator for having worked in the previous week; *Work Status* is the kind of work with seven different categories; *Agreeableness*, *Extraversion*, *Conscientiousness*, *Neuroticism*, and *Openness* form the BIG Five personality traits.

Entrepreneurs out of opportunity have a significantly higher level of education than entrepreneurs of necessity and all the other groups. Furthermore, they are more likely to be female and tend to be less neurotic than necessity entrepreneurs. There are no other significant differences between the two groups. In comparison to return migrants, entrepreneurs are generally less likely to be male, but

[11] Our results are qualitatively robust to excluding those persons. However, sample size issues render some effects insignificant and less precise. Results are available upon request.

[12] Two observations are excluded from the analysis because these participants did not complete the whole experimental session.

better educated and more likely to have worked in the previous week. Strikingly, entrepreneurs also exhibit a significantly higher level of extraversion and conscientiousness as well as a lower level of neuroticism. These differences are especially driven by entrepreneurs out of opportunity. Entrepreneurs also have a higher level of education and are more likely to work than the remaining sample. Additionally, they are slightly older and more male and have a higher level of extraversion; these differences are again mostly driven by opportunity entrepreneurs. In general, opportunity entrepreneurs show the lowest levels of neuroticism. In Table 4.2, descriptive statistics across treatment and control group are compared.

Table 4.2: Descriptive Statistics across Treatments

	Full Sample	Control	Treatment	Difference
Male	0.54	0.45	0.62	-0.16^{**}
Age	34.28	33.89	34.66	−0.77
Education	5.44	5.26	5.61	−0.34
Dummy Working	0.51	0.45	0.56	−0.11
Work Status	2.38	2.58	2.22	0.36
Index b	0.03	0.03	0.02	0.01
Index a	0.69	0.71	0.67	0.04
Sigma	1.02	1.06	0.98	0.08
Alpha	0.70	0.67	0.72	−0.05
Remain	0.40	0.40	0.40	−0.00
Ent.	0.33	0.32	0.34	−0.02
Nec.	0.15	0.13	0.18	−0.05
Opp.	0.18	0.19	0.16	0.03
Return	0.27	0.28	0.26	0.02
Observations	222	110	112	222

Variables: *Male* is an indicator for being male or female; *Age* is the age of the participant in years; *Education* is a categorical variable from 1-9, where 1 is "no degree/no education" and 9 is "doctoral degree or equivalent;" *Dummy Working* is an indicator for having worked in the previous week; *Work Status* is the employment status with seven different categories; *Index b and Index a* are ambiguity attitudes derived in Section 4.2.2; *Sigma and Alpha* are risk attitudes; *Ent.* are all entrepreneurs; *Nec.* and *Opp.* only include entrepreneurs out of necessity and opportunity respectively; *Return* represents the group of returnees and *Remain* includes all persons who are neither entrepreneurs nor returnees.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

There is an imbalance in the sex composition as the share of males in the treatment group is significantly higher. However, all other variables, especially ambiguity attitudes and our groups of interest, are well balanced. An f-test on

the joint significance of all variables reveals that the controls are not related to treatment assignment (p-value: 0.762).

4.3 Results

We first discuss the prevalence of ambiguity aversion and a-insensitivity in the whole sample and in our groups of interest. Subsequently, we discuss whether there are significant differences between these groups. In the second part, we analyze the treatment and its effects on choices under uncertainty, as formulated in hypotheses 1-6.

4.3.1 Ambiguity Aversion and A-Insensitivity

Table 4.3 summarizes the ambiguity aversion measures. Correlations between our parameters, estimated with the methods introduced in Section 4.2.2, are in Appendix Table C.1.1.[13] Naturally, index a, which measures a-insensitivity, is correlated to the local ambiguity parameters for low and high probabilities because they are used to construct the index. However, there is only a weak correlation between ambiguity aversion and a-insensitivity. This is consistent with previous studies (see Dimmock et al., 2016) and the notion that ambiguity aversion and a-insensitivity are two distinct preferences induced by ambiguity.

Table 4.3: Summary Ambiguity Parameter

	Mean	Median	Std. Dev.	Min.	Max.
Matching Prob. m(0.1)	0.36	0.21	0.31	0.01	0.89
Matching Prob. m(0.5)	0.50	0.44	0.30	0.06	0.94
Matching Prob. m(0.9)	0.60	0.56	0.33	0.11	0.99
$AA_{0.1}$	-0.26	-0.11	0.31	-0.79	0.09
$AA_{0.5}$	0.00	0.06	0.30	-0.44	0.44
$AA_{0.9}$	0.30	0.34	0.33	-0.09	0.79
Index b	0.03	0.04	0.51	-0.88	0.88
Index a	0.69	0.84	0.41	-0.23	1.98

m(0.1), m(0.5), and m(0.9) are the matching probabilities derived from the three ambiguity choice sets. $AA_{0.1}, AA_{0.5}$, *and* $AA_{0.9}$ are the differences between objective and matching probabilities-the local ambiguity attitudes. *Index b and a* are global indices for ambiguity aversion and a-insensitivity derived via linear approximation.

[13] In this Table, it is also seen that the matching probabilities approach is highly correlated to measuring ambiguity aversion with the Ellsberg problem, which supports the validity of matching probabilities.

Our sample is, on average, ambiguity neutral. This is indicated by index b being, on average, almost equal to zero and is in contrast to similar studies done in other countries, where the average study participant is ambiguity averse (for an overview see Trautmann and van de Kuilen, 2015). Ambiguity aversion seems to be less prevalent in our sample than in previous studies, which are mostly conducted in industrialized countries or in developing economies with small-scale farmers.

This lower rate of ambiguity aversion is not exclusively driven by our sample composition. We explicitly focus on groups of the population that we hypothesize to be more ambiguity tolerant, namely entrepreneurs and return migrants. The share of ambiguity averse persons is lower within the groups of necessity entrepreneurs and returnees; however, this is not significantly different from the remaining sample. Furthermore, even for the remaining sample the share of ambiguity averse subjects is still below 50%. Given the results from the previous literature, this is already an interesting finding in itself.

In Table 4.4, we do not find much differences between necessity and opportunity entrepreneurs with regard to ambiguity aversion. If anything, necessity entrepreneurs are less ambiguity averse for moderate probabilities (onesided t-test, p=0.087). They also seem to be slightly more ambiguity seeking for small probabilities in comparison to the remaining sample, whereas opportunity entrepreneurs have the tendency to actually be more ambiguity averse for large probabilities.

Table 4.4: t-tests - Differences in Ambiguity Parameters

	Diff. Nec.-Opp.	Diff. Rem.-Nec.	Diff. Rem.-Opp.	Diff. Ret.-Nec.	Diff. Ret.-Opp.
$AA_{0.1}$	−0.06	0.07	0.01	0.11*	0.05
$AA_{0.5}$	0.09*	−0.03	0.05	0.02	0.11**
$AA_{0.9}$	0.04	0.02	0.06	−0.02	0.02
Index b	0.04	0.04	0.08	0.08	0.12
Index a	0.12*	−0.06	0.06	−0.16**	−0.03
Observations	73	123	128	94	99

Groups: *Diff. Nec.-Opp.*, *Diff. Rem.-Nec.*, *Diff. Rem.-Opp.*, *Diff. Ret.-Nec.* and *Diff. Ret.-Opp.* are various differences between necessity entrepreneurs, opportunity entrepreneurs, returnees, and the remaining sample. Variables: $AA_{0.1}$, $AA_{0.5}$ *and* $AA_{0.9}$ are the local ambiguity attitudes. *Index b and a* are global indices for ambiguity aversion and a-insensitivity derived via linear approximation. Positive differences indicate that value of the parameter is larger for the second group in each pair.

Onesided t-tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

However, none of these differences are significant at conventional levels. Yet, we do find significant differences between entrepreneurs and return migrants. Returnees are more ambiguity seeking than necessity entrepreneurs for small probabilities and more ambiguity seeking for moderate probabilities than opportunity entrepreneurs. Returnees and entrepreneurs seem to differ from the general population in opposing directions. Thus, studies that try to test for differences in risk attitudes between entrepreneurs and non-entrepreneurs should take the share of migrants in their sample into account depending on their research question.

For a-insensitivity, we find necessity entrepreneurs to be significantly less a-insensitive than opportunity entrepreneurs and return migrants. The highest risk-takers (as they have other outside options in contrast to the perception of necessity entrepreneurs) are those who discriminate the least between different probabilities and tend to treat every level of uncertainty as a 50-50 chance. There is, however, no significant difference between the remaining sample and entrepreneurs of all kinds.[14]

Overall, we do not find very pronounced differences between the two kinds of entrepreneurs and the other groups. This also does not change if we control for further potential covariates. In Appendix 5.5, we provide regression results including covariates and discuss if the significant covariates in our sample differ from those in former studies. For ambiguity aversion, our results are in line with studies concluding that, although theoretically appealing, there might be no relationship between greater risk tolerance and entrepreneurship (see for example Astebro et al., 2014, for an overview).

Our Albanian and Kosovar participants are, in general, slightly less ambiguity averse but more a-insensitive than already studied populations. The overweighting of small probabilities might imply, in general, a greater willingness to migrate and open a business already. Still, we find that necessity entrepreneurs are less a-insensitive than entrepreneurs out of opportunity and other risks groups. It seems that the perception of the size of uncertainty is the basis on which both kinds of entrepreneurs differ rather than the level of ambiguity aversion, if uncertainty is given by nature. Some studies find that the perception of and the tolerance toward strategic uncertainty (Holm et al., 2013) are stronger predictors of entrepreneurship. We look closer at strategic uncertainty and the effect of perceived uncertainty with our results for the treatment in the following section.

[14] In general, contrary to the case of ambiguity aversion, the average person in our sample is more a-insensitive than other populations (e.g the Dutch in Dimmock et al., 2016). This is driven by both components almost equally, the stronger overweighting of small probabilities and the stronger underweighting of high probabilities.

4.3.2 Competence Treatment and Choices under Uncertainty

At first, we concentrate on hypotheses one to four and look at the uncontrolled differences between control and treatment groups for the different choice sets. A onesided t-test reveals that the difference between control and treatment in the certainty equivalent for non-strategic uncertainty is significant at the 10% level (p=0.068). However, we do not find a statistically significant effect for the certainty equivalent for strategic uncertainty (onesided p=0.29). Fisher's exact tests for the share of CE's above 150 (which would be the "ambiguity-neutral," expected value of a 50-50 bucket) show the same pattern (onesided p-values: 0.047 and 0.347). This is surprising and a sign that respondents are more open to uncertainty because of a positive experience with uncertainty but not specifically in the domain of strategic uncertainty. The conjecture is further supported by the significant, positive difference in the direct choice between strategic and non-strategic uncertainty (p=0.064). Participants in the treatment group become more tolerant toward non-strategic uncertainty instead of preferring strategic uncertainty.[15] This is also seen in Figures 4.5 and 4.6.

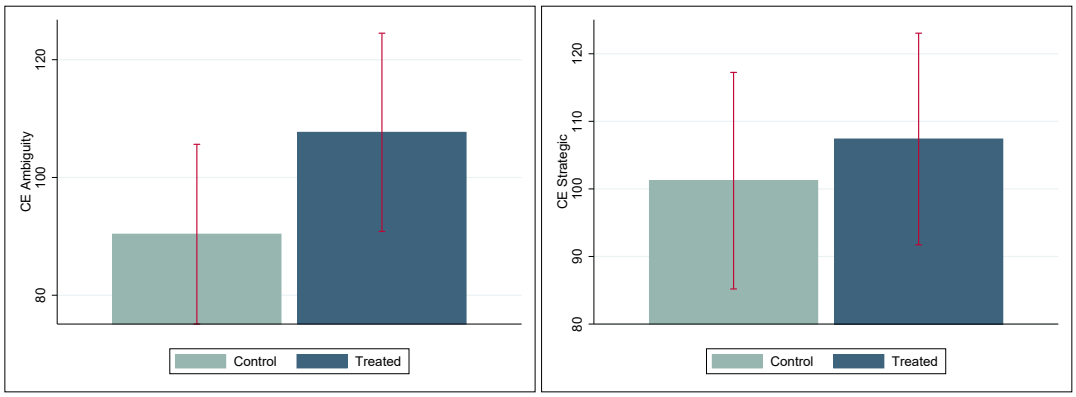


Figure 4.5: CE's for Non-Strategic (left) and Strategic (right) Uncertainty by Treatment

The same pattern also holds for the fourth choice (see right picture of Figure 4.6). Participants in the treatment group are more willing to "trust" other persons and let them make decisions (onesided t-test, p=0.053). Consequently, we reject hypotheses one and three because the significant differences are not in the direction we expected. We cannot reject hypothesis two and four although,

[15] We are generally slightly under-powered for these comparisons as our effect sizes are fairly small. We are able to detect moderate effect sizes (0.33) with our sample size (means test, alpha of 0.05, onesided and a power of 0.8). However, the effect size of our null-result (0.07) is also economically not meaningful.

unexpectedly, the effect on the non-strategic CE is larger than the one on the strategic CE. Combining these initial results, it seems that the treatment increases the tolerance to the more ambiguous option in each choice set only if ambiguity originally derives from nature.

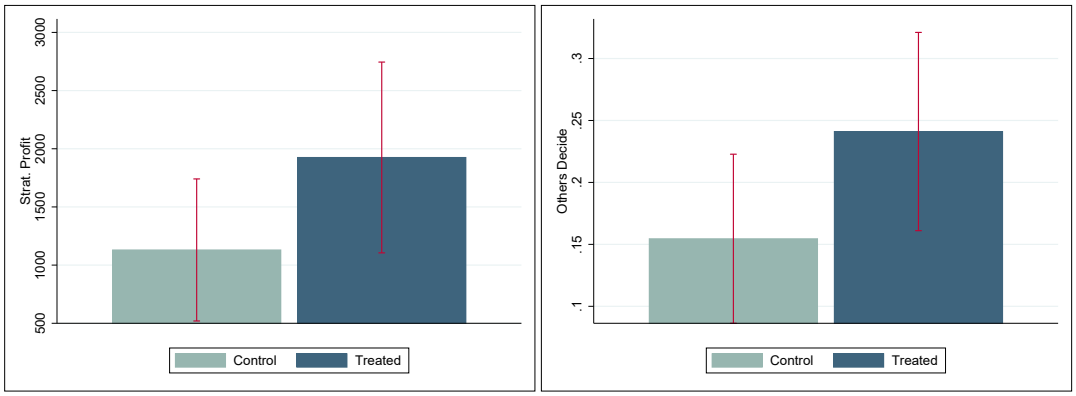


Figure 4.6: Strategic vs. Non-Strategic Uncertainty (left) and Taking Responsibility (right) by Treatment

Looking at hypothesis 5 and 6, we first test whether there are already significant differences between entrepreneurs and returnees at baseline by comparing results across groups for those in the control treatment. Results are shown in Appendix Table C.1.2.[16]

Necessity entrepreneurs seem to dislike non-strategic uncertainty more than opportunity entrepreneurs, however not strategic uncertainty. The certainty equivalent for non-strategic uncertainty and the willingness to accept non-strategic uncertainty in comparison to strategic uncertainty are much larger for opportunity entrepreneurs, although not significantly larger.

This result is not driven by outliers and supports the notion that necessity entrepreneurs are as willing as opportunity entrepreneurs to accept strategic uncertainty but not non-strategic uncertainty. There is no difference in letting others decide between the two kinds of entrepreneurs; however, they are both much less likely to lay their fate in others' hand than return migrants at baseline. Out of all groups, entrepreneurs out of necessity are the ones who are the least willing to tolerate non-strategic uncertainty, indicated by the several large differences between them and the other groups. In general, returnees seem to tolerate non-strategic uncertainty in isolation more than both kinds of entrepreneurs.

Table 4.5 presents the differences (and onesided significance) between treatment and control for each group of interest separately. For the whole group of

[16] The sample sizes for this exercise are small, so these tests are under-powered but we still find it worthwhile to look at initial differences to identify small effect sizes.

entrepreneurs, we find large effects for all choices except the CE for strategic uncertainty. The treatment significantly increases their certainty equivalent for non-strategic uncertainty by 33.95 points, which are about 0.5 standard deviations, and their willingness to let others decide by 15 percentage points, which is a tremendous increase since the baseline probability is 5.5%.

Table 4.5: Individual t-tests for Groups of Interest

	Entrepreneurs			Return	Remain
	All	Nec.	Opp.		
CE Ambiguity	33.95**	53.26***	20.89	−6.23	19.93
CE Competition	11.90	22.78	−1.33	3.58	3.12
Competition vs. Ambiguity	734.09	1684.67**	98.43	968.74	712.28
Others Decide	0.15**	0.08	0.23**	0.02	0.09
Observations	73	34	39	60	89

Groups: *All* are all entrepreneurs; *Nec.*, and *Opp.* include entrepreneurs out of necessity and opportunity respectively; *Return* represents the group of returnees and *Remain* includes all persons who are neither entrepreneurs nor returnees. Variables: *CE Ambiguity* is the outcome for the certainty equivalent of non-strategic uncertainty; *CE Competition* is the outcome for the certainty equivalent of strategic uncertainty; *Competition vs. Ambiguity* is the outcome for the total profit in the strategic uncertainty option against non-strategic uncertainty; and *Others Decide* is the outcome for the probability of letting other decide the winning numbers instead of oneself. Positive differences indicate that value of the parameter is larger for the first group in each pair of groups.

Onesided t-tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Strikingly, the increase in tolerance for non-strategic uncertainty is mostly driven by entrepreneurs out of necessity. The treatment more than doubles the certainty equivalent for non-strategic uncertainty. Furthermore, the difference is tremendously large for choosing non-strategic in favor of strategic uncertainty. In contrast, entrepreneurs out of opportunity only respond weakly in every choice, if not in a negative direction. Both kinds of entrepreneurs are more willing to let others decide but only opportunity entrepreneurs are significantly more likely, by 23 percentage points. In general, as necessity entrepreneurs respond much more to the treatment in the directions hypothesized than opportunity entrepreneurs, we cannot reject hypothesis five.

On average, return migrants only weakly respond to the treatment, especially for the certainty equivalents and for letting others decide. The effect for non-strategic uncertainty is even negative, albeit small. It is worth mentioning that, at baseline, returnees are significantly more willing to let other persons decide than

entrepreneurs and that the positive treatment effect for opportunity entrepreneurs just puts them on the same level as returnees. Summarized, we cannot reject hypothesis six that entrepreneurs respond more strongly to the treatment than do returnees.

So far, we have not discussed the correlation between the ambiguity attitudes elicited in part two (Section 4.2.2) and the choice sets in part four (Section 4.2.3) of our experiment. In general, the correlation between different elicitation methods for ambiguity attitudes seem to be rather weak. Trautmann et al. (2011), for example, find substantially more ambiguity aversion and even preference reversals for willingness-to-pay tasks in comparison to choice tasks, which we use. We have chosen these specific tasks because (i) they have a solid decision-theoretic foundation; and (ii) because we want to elicit a-insensitivity explicitly. The choice sets we implement in part four of the experiment are not suited to measure ambiguity aversion. The idea of these choice sets is to analyze whether a treatment that is supposed to change competence and familiarity affects the willingness to take uncertain gambles in general, not in comparison to risk. Our assumption is that choices in these sets are much more driven by beliefs than by ambiguity aversion. Furthermore, choices that are made after the treatment intervention are potentially altered by the treatment. This is also true for our control group, as they are made aware of the fact that their judgement was not correct. Hence, a comparison of choices between the two tasks to assess the quality of the elicitation method might be misleading.

However, it is still interesting if ambiguity aversion parameters affect choices and interact with the treatment. In Appendix Tables C.1.3 and C.1.4, correlations between the global ambiguity index, a-insensitivity, and the choice sets for both treatment groups are depicted. For the control group, not only is there no significant correlation between ambiguity aversion and any of the choices, but there is only one significant correlation between a-insensitivity and the choice between strategic and non-strategic uncertainty. In contrast, for the treatment group, we find a positive correlation between ambiguity aversion and both certainty equivalents. A-insensitivity has no significant correlation to any choice. We interpret this as a sign that the competence treatment especially affected those with a higher level of ambiguity aversion.

Summarized, we confirm three out of the five hypotheses, in particular that necessity entrepreneurs react more to the treatment than opportunity entrepreneurs. At baseline, necessity entrepreneurs are much less willing than either opportu-

nity entrepreneurs or return migrants to accept non-strategic uncertainty, which is subsequently more than off-set by the treatment. However, there is no strong baseline difference regarding strategic uncertainty, which is an interesting result. In general, both kinds of entrepreneurs respond strongly to a rather mild treatment as opportunity entrepreneurs' willingness to let others decide also increases significantly. Comparing this to another group of risk-takers, returnees, hints at the fact that this is not driven just by different degrees of ambiguity aversion as they do not differ substantially in this respect. Still, it should be noted that the groups experiencing the largest boost in taking uncertain gambles is the one that is the least a-insensitive.

Generally, our evidence is not directly in favor of the hypothesis that an increase in perceived domain-specific competence drives these results. Although our treatment intends to manipulate the perceived competence of the participants to judge other participants, mostly the willingness to tolerate non-strategic uncertainty increased. Surprisingly, it seems that the treatment affected the general level of optimism. In all four choices, participants in the treatment seem to be more optimistic toward the option that entails larger uncertainty.[17] Further evidence for the conjecture that the treatment effected general optimism is shown in Appendix Figure C.1.1. Directly after the treatment message, participants are also asked how well they think they did in the previous parts in comparison to other participants in the room. They should place themselves between one and ten, where one stands for the person who earned the fewest points so far and ten for the person who earned the most. The performance in the previous tasks is only weakly linked to guessing the number of competitors correctly as most choices, like the one outlined in 4.2.2, do not entail any strategic uncertainty. Still, those in the treatment group ranked themselves higher than those in control (onesided t-test, p-value:0.087); hence expecting to have performed better so far. We see this as additional evidence that the level of optimism and not competence is spurred.

4.4 Robustness

Controlling for the Order of Choices. There is reason to believe that the order in which the three uncertainty choices are played could matter. Fox and Tversky (1995) are among the first who noticed that ambiguity aversion is much more

[17] The choice regarding deciding on one's own or let others decide in principle bears the same risk. However, as outlined in Section 4.2.3, participants might perceive letting others persons decide as more ambiguous.

pronounced if the ambiguous prospect can be compared to a less ambiguous one and is not considered in isolation. Their "comparative ignorance hypothesis" also explains why people prefer to bet on ambiguous prospects in areas they feel competent about than on ambiguous prospects in areas where they do not have knowledge or experience. Thus, the order in which the choices are considered might change the results. In Table C.1.5 in the Appendix, we control for each of the three choices whether it is elicited first, second, or third. As can be seen, the effects and the coefficients for the treatment stay almost the same. This also holds for our groups of interest individually (results upon request). Interestingly, the order still has a significant effect on the direct choice between strategic and non-strategic uncertainty. The later it is elicited, the less participants are attracted to the non-strategic option.

Controlling for Certainty about the Guess. As mentioned in Section 4.2.3, participants are not only asked to guess the number of competitors in their group but also how certain they are about this guess on scale from one to ten. Extreme certainty or uncertainty could "interfere" with our treatment in various ways. Those who are extremely certain could not react to the treatment at all or are shattered if they learn they guessed incorrectly. Those who are extremely uncertain could experience the biggest boost in competence if they are treated. Thus, it is not clear *ex ante* whether certainty and our treatment are complements or substitutes, especially since our treatment rather affected the perception of non-strategic uncertainty. Therefore, in Appendix Table C.1.6, the certainty variable is included as control. As before, our treatment effect is robust to this inclusion (also in the individual groups of interest). What is striking is the comparative ignorance effect we apparently find. The certainty equivalent choices where the two ambiguous prospects are considered in isolation are almost not affected by how certain the participants are. The direct choice between the strategic and non-strategic uncertainty, however, largely and significantly depends on certainty. Those who are more certain about their guessed number of competitors seem to be much more willing to bet on the strategic gamble. This holds for participants in the treatment and the control groups. Hence, our treatment and the perceived certainty (which can be a form of perceived competence) are rather substitutes as their effects go in opposite directions. This again underlines the conjecture that the treatment affected general optimism and not competence. Interestingly, necessity entrepreneurs are significantly more certain than opportunity entrepreneurs

(twosided p-value=0.05) and are minimally more tolerant to strategic uncertainty at baseline.

4.5 Conclusion

The observed prevalence of entrepreneurship is sometimes hard to reconcile with conventional levels of risk taking measured in standard models. On the other hand, in weak labor markets, individuals might feel that they have no option other than becoming self-employed. Motivated by the mixed findings on whether entrepreneurs are more risk seeking than non-entrepreneurs and by different motives to become entrepreneurs in the first place for necessity and opportunity entrepreneurs, we analyze if the latter groups differ with respect to their level of ambiguity aversion, a-insensitivity, and the willingness to take non-strategic and strategic, uncertain gambles. Additionally, we isolate return migrants from the remaining general population to compare entrepreneurs to another group of occupationally high-risk takers, and to make sure the aforementioned mixed findings are not driven by a varying share of migrants in the comparison group.

Furthermore, as theory predicts that expected returns to entrepreneurship also depend on (perceived) competence, we randomize a competence treatment. The treatment exogenously strengthens the perceived ability to judge other persons' market entry decision for half of our participants before we measure the willingness to take uncertain gambles. Differences in perceived uncertainty and competence could explain different entry rates and why "necessity businesses" are less profitable than "opportunity businesses." We expect necessity entrepreneurs to be more responsive to treatment than opportunity entrepreneurs and entrepreneurs in general to be more responsive than return migrants. Our sample consists of real entrepreneurs, returnees, and other persons living in Albania and Kosovo, as middle-income countries particularly rely on innovation as a source of growth.

Using the matching probabilities method by Dimmock et al. (2016), we find that although there are no substantial differences in ambiguity aversion, necessity entrepreneurs are significantly less a-insensitive than opportunity entrepreneurs and return migrants. The latter two groups seem to have a different perception of non-strategic uncertainty, because they tend to consider all unknown probabilities as 50-50 chances. Our results also show that, in general, necessity entrepreneurs are the least willing to take non-strategic, uncertain gambles but subsequently, respond strongest to the competence treatment. They experience a large increase in the willingness to take non-strategic, not so much to take strategic uncertain

gambles. This hints at the fact that it is not perceived competence that is altered but rather general optimism. In contrast, returnees do not respond well to the treatment, while opportunity entrepreneurs only respond in trusting other people more. Our results are robust to order effects and participants' pre-treatment certainty about their ability to judge the decision of others.

With our study, we contribute to the literature trying to shed light on the behavioral determinants of entrepreneurship and how motives behind opening a business potentially shape business outcomes. Behavioral factors can be important barriers for market entry and firm survival, which policy makers want to resolve. Boosting confidence or optimism seems to help necessity entrepreneurs in increasing their willingness to gamble on non-strategic uncertainty, which might be beneficial for innovation and funding. For example, Cusolito et al. (2020) argue that entrepreneurs not accepting venture capital might be one of the reasons for start-up failure in the Western Balkans. Additionally, they find that inexperienced and small firms especially profit from a business readiness program that emphasizes entrepreneurial skills. Besides actually improving skills, another reason for their success could be the increased feeling of competence and the subsequent increase in willingness to gamble on uncertainty. At least in our experiment, this increase is still below levels that expected utility theory would deem optimal and are, in contrast to the assumption in some theoretical studies, not irrationally high.

Chapter 5

Don't Expect Too Much - High Income Expectations and Over-Indebtedness

We thank Anik Ashraf, Jana Friedrichsen, Antonia Grohmann, Stefan Klonner, Friederike Lenel, Lukas Menkhoff, Marina Schröder, Lisa Spantig, Susan Steiner, Sidiki Soubeiga, Fabian Stöckl, Stephan Thomsen, Andreas Wagener, Georg Weizsäcker, and seminar participants in Berlin, Göttingen, Hannover, Potsdam and, Tutzing for helpful comments. The paper also profited from discussions with participants at ESA World 2018 (Berlin), GLAD Conference 2018 (Göttingen), the 2019 AEL-FHM Doctoral Meeting (Mannheim), the 6th PhD Workshop on Experimental Development Economics at UEA (Norwich), SSES Annual Congress 2019 (Geneva), NCDE 2019 (Kopenhagen), the 5th Dial Conference on Development Economics (Paris), EEA 2019 (Manchester), VfS Annual Conference 2019 (Leipzig), IAREP/SABE 2019 (Dublin), and ASSA 2020 (San Diego). We are grateful to Hanh Le Van, Inka Nozinski, and Theerayuth Labooth, who provided excellent research assistance. Financial support by the German Research Foundation (DFG) via the Research Training Group 1723, the TVSEP Project (Project No. 283672937) and CRC TRR 190 (Project No. 280092119) is gratefully acknowledged.

5.1 Introduction

For households, taking out debt is a valuable tool to smooth consumption and often a necessary precursor of private investments. However, as consumer indebtedness is significantly increasing worldwide, there is widespread concern that it may turn detrimental. Specifically, when households face increasing difficulties to repay their debts, household well-being and consumption are threatened. Moreover, household over-indebtedness poses a serious threat to the stability of the financial system as a whole; for example, as experienced during the U.S. financial crisis in 2007-08.

Emerging market economies are especially at risk of low growth and even financial crises when the level of household debt is high, as not only are their institutions and financial regulations weaker, but income inequality is also higher (IMF, 2017). Therefore, understanding the factors and reacting to the consequences of over-indebtedness are crucial for improving living conditions while also ensuring a stable development of emerging economies. Building on the "permanent income hypothesis", where income expectations determine current consumption and borrowing, this paper studies one potential driver of over-indebtedness: too high income expectations. Although being positive about the future might have a net positive effect on lifetime utility (see Brunnermeier and Parker, 2005), being too positive might lead to serious financial distress and over-indebtedness.

In general, households' borrowing behavior around the world is still puzzling in various aspects and often hard to reconcile with standard neoclassical and behavioral models. Zinman (2015) argues that one reason for many unresolved puzzles is that household debt is vastly under-researched within household finance. In the last decade, a vibrant literature on measuring over-indebtedness has emerged (e.g. D'Alessio and Iezzi, 2013; Keese, 2012; Schicks, 2013). In contrast, its determinants are still mostly unidentified. Our paper contributes to closing this gap by focusing on high income expectations as one likely cause. To the best of our knowledge, we are the first to study the relationship between real income expectations and over-indebtedness.

We investigate the relationship between positive expectations and over-indebtedness using extensive survey data on the financial situation and financial behavior of one of the most vulnerable populations in Thailand: rural households in the north-east. A crucial part of our survey was to collect objective and subjective data on potential symptoms of over-indebtedness. This allows us to construct

different objective and subjective over-indebtedness indicators.[1] Additionally, we quantify households' predictions of their future income. Instead of relying on qualitative Likert scale measures, we elicit individual distributions of expected household income and set these in relation to actual income. Hence, a major contribution to the literature is that we relate the over-indebtedness indicators to a sophisticated measure of subjective income expectations. In our regression analysis, we control for relevant household characteristics and unexpected shocks faced by households, thereby reducing reverse causality concerns. In order to further strengthen the contribution of our paper, we delve deeper into the causal effect of positively biased expectations on overborrowing by carrying out a lab-in-the-field experiment with the exact same respondents. In the experiment, we concentrate on one particular expectation bias: overconfidence. We exogenously bias income expectations via two treatments that vary the level of self-confidence of the respondents and, thereby, their expected earnings. Subsequently, we investigate if participants spend more on goods they can buy in the experiment and, as a consequence, potentially overborrow.

Thailand is, on the one hand, an exemplary emerging market, but, on the other, outstanding when it comes to household finances: Financial inclusion is comparatively high, with four out of five persons participating in the formal financial system. Simultaneously, household debt has increased to over 78.03% of the country's GDP. This makes it the emerging market with the highest household debt to GDP ratio in the world (IMF (2017), see Appendix Figure D.1.1). Given these numbers, it is hardly surprising that both local policy makers and international institutions agree that over-indebtedness is a growing problem in Thailand (Tambunlertchai, 2015). Additionally, there are circumstances that make our sample especially vulnerable to over-indebtedness and to struggle with financial hardship. This part of the population faces higher uncertainty regarding their future incomes in two ways: through the generally high level of macroeconomic volatility in emerging markets and through individual, mostly weather-related shocks, common to poor, small-scale agricultural households (see Loayza et al., 2007; Klasen and Waibel, 2015).

Our survey results show that there is a strong and robust relationship between high expectations and over-indebtedness. Those who have positive expectations are more likely to be over-indebted than those with neutral or negative expectations, which we interpret as a sign that these expectations are truly too

[1] It is still a highly debated topic how to measure over-indebtedness and there is no clear-cut answer on the right method of elicitation, which is why we construct a variety of over-indebtedness measures.

high for some households. The results vary slightly with respect to different debt indicators. The relationship between high expectations and the objective over-indebtedness indicator is more pronounced in comparison to the subjective indicator, but both relationships are significant. Our results indicate that the subjective indicator is not only driven by actual debt levels but also by personal characteristics and perceptions, such that it measures a different dimension of over-indebtedness. In an additional exercise, we can show that the subjective over-indebtedness indicator is highly correlated to a qualitatively assessed income forecast (error) measure. Eventually, we find that being more certain about the future income realization, which can be another form of forecast error, is also positively related to our objective over-indebtedness indicator. Rural households are exposed to a highly uncertain environment; hence, being too certain about ones future income may be harmful. Our results are robust to various sample specifications and become more precise if we exclude parts of the sample that may have had difficulties understanding the questions on eliciting future income expectations.

In the supplemental experiment, we find that overconfidence is related to more spending and overborrowing. However, our treatments themselves have no impact on overborrowing, which is why we cannot claim a causal relationship of overconfidence on overborrowing. These results are not driven by presumably confounding factors that the treatments could have affected and are relatively robust. Rather, we find evidence for "sticky" overconfident beliefs, which also points to a high level of perceived certainty in our sample. Furthermore, participants who overspend in the lab are also those who experience over-indebtedness in real life. This shows that our experiment is not "too artificial" to capture real life behavior.

Our study touches on three strands of literature: First, the literature on eliciting and using subjective expectations data; second, research on potential behavioral biases in financial decision-making and debt illiteracy; and, third, the literature on households' (over-)indebtedness in emerging economies. There are at least two reasons why the relationship between income expectations and over-indebtedness should be explicitly studied in an emerging market setting and why findings from "WEIRD"[2] populations might not translate to rural populations. First, financial literacy is substantially lower. This implies lower debt literacy, which might hamper expectation formation on financial matters. For example, Lusardi and Tufano (2015) find that debt illiteracy is related to higher debt

[2] Western, educated, industrialized, rich and democratic

burdens and the inability to evaluate the own debt position. Burke and Manz (2014) experimentally show that economic illiteracy increases financial forecast errors. Second, the higher uncertainty that respondents are facing distinguishes this research from work done in "WEIRD" societies. A more volatile economic environment requires more individual belief formation, which makes biased expectation formation more likely (see for example Johnson and Fowler, 2011) and at the same time more dangerous. In any case, the empirical evidence from WEIRD countries on the relationship between income expectations and over-indebtedness is sparse as well. To the best of our knowledge, there is no study that explicitly concentrates on real-life income expectations.

Our work is most closely related to Hyytinen and Putkuri (2018) and Grohmann et al. (2019). The former find a correlation between Finnish households' overborrowing and extreme positive forecast errors about the financial situation of the household. They do not analyze the effect of income expectations on overborrowing but the effect of financial expectations in general, which gives more rise to issues of reverse causality. Furthermore, the forecast errors are constructed using Likert scales and hence, cannot be quantified. They show that households exhibiting high positive forecast errors are more likely to overborrow than households exhibiting smaller errors. Grohmann et al. (2019) conduct a lab experiment among students in Germany that is similar to ours and link the experiment data with data from the German Socio-Economic Panel. They find a causal link between overconfidence and debt taking in the lab and a correlation between a simple measure for overconfidence and the level of household debt in the panel sample. Our study differs from these two studies in that it contributes to the literature by (i) explicitly eliciting and quantifying real income expectations and precisely measuring over-indebtedness; and (ii) analyzing the research question in a setting where expectation formation is generally difficult and over-indebtedness bears severe consequences.

The paper proceeds as follows: Section 5.2 presents the survey data, discusses the setting, and explains how our variables of interest are constructed. In Section 5.3, the estimation strategy is outlined and survey results are presented. Section 5.4 describes the experiment and its results. Section 5.5 concludes.

5.2 Data

This section introduces the data collected during the survey and explains how the main variables of interest are derived. We develop a measure that approximates

future income expectations, which we call the quantitative income forecast. Further, we construct various over-indebtedness indicators to capture the different dimensions of household debt.

5.2.1 The Thailand Vietnam Socio Economic Panel

The survey was conducted in Thailand in November 2017 and is an add-on project of the Thailand Vietnam Socio Economic Panel (TVSEP).[3] The TVSEP has conducted panel surveys in rural Thailand and Vietnam on a regular basis since 2007, with recurrent surveys in 2008, 2010, 2011, 2013, 2016, 2017, and 2019, so far. The TVSEP survey captures the living conditions of households in rural areas that are largely engaged in agriculture. It focuses on factors affecting households' vulnerability to poverty. Among others, the survey includes socio-economic characteristics of every household member, sections on household consumption and savings, crop farming, livestock rearing, and, in particular, questions on exposure to shocks and anticipated risks. Furthermore, each wave captures topics of current research interest. About 4000 rural households in 440 villages across six provinces in Thailand and Vietnam are interviewed for the survey. The sample is set to represent the rural population in these two countries while urban households are deliberately excluded. To obtain a representative sample, a three-stage cluster sampling is used. The procedure is described in Hardeweg et al. (2013).

Our study is conducted in only one of the TVSEP provinces in Thailand, Ubon Ratchathani, which borders Cambodia and Laos (Figure 5.1). The sample consists of about 750 households in 97 villages. For the majority of our analysis, we concentrate on our own survey, adding data from the 2016 and 2017 general TVSEP survey as necessary. With our study, we want to gain new insights into the determinants of debt induced financial distress within a vulnerable population. Therefore, our survey includes extensive question batteries on objective and subjective over-indebtedness (see Sub-Section 5.2.4), savings, financial literacy, borrowing behavior in general, and income expectations (see Sub-Section 5.2.3). In addition, we collect data on health, subjective well-being, personality traits, and risk preferences. We use established items to assess these data. For example, personality traits are measured using the short version of the Big Five Inventory "BFI-S" (John and Srivastava, 1999; Gerlitz and Schupp, 2005). We develop a broad financial literacy score, which not only encompasses numeracy but also questions on financial behavior and attitude. The score is similar in style to that developed by the OECD (OECD, 2018).

[3] See https://www.tvsep.de/overview-tvsep.html.

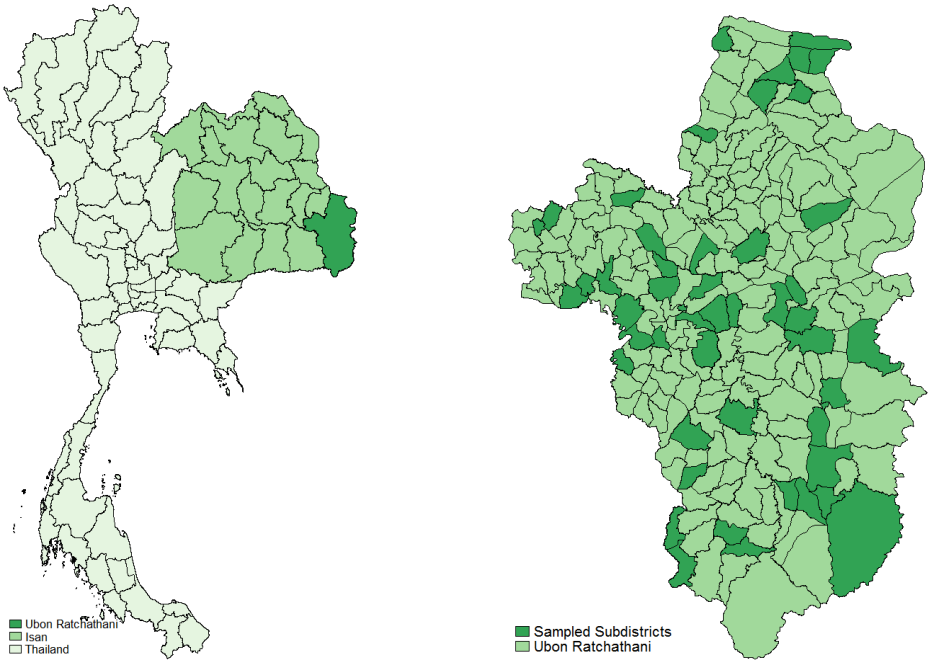


Figure 5.1: Study Site, Ubon Ratchathani and Sampled Subdistricts

Furthermore, we construct a score for risk preference out of two questions: The first one asks whether the person is generally fully prepared to take risks and the second question specifically asks for risk-taking behavior in financial decision-making (i.e. investing and borrowing). Self-control is assessed using the well-established scale of Tangney et al. (2004). Given the low numeracy within the sample, we add a phrase to each numerical value on questions involving scales.[4]

We use a restricted sample for the analysis in Section 5.3 and exclude outliers by the following means: We exclude (i) the 1 percent highest monthly household incomes in 2016 and 2017, (ii) households who have a debt service to income ratio greater than four, and (iii) those whose income is negative in general. For the latter case, we trim them as we do not know whether a negative income itself means that the households are in financial distress. Regression results without trimming are very similar to those with trimming. In any case, trimming (marginally) downward biases our results.

In our trimmed sample, the average respondent is 57 years old, female, the spouse of the household head, and has 5.7 years of education. Our financial literacy score indicates a relatively low level of financial literacy. On average, respondents answered four out of seven knowledge questions correctly, reached five out of nine possible points concerning financial behavior, and three out of

[4] Our main questionnaire can be downloaded here.

seven possible points with regard to financial attitude. This is in line with findings from the OECD/INFE study for Thailand from 2016 (OECD, 2016). While 57.27% of our respondents are the sole financial decision makers in their households, 28.05% share this task with someone else. Hence, when sometimes using respondent- and not household-specific characteristics or perceptions in the analysis, we are still confident that these individual traits determine the household's state of indebtedness because the majority of respondents is in charge of making financial decisions.[5]

5.2.2 The Thai Rural Credit Market

In Thailand, over 80% of the population has a bank account and over 60% uses it for digital payments. The gaps in financial inclusion between women and men as well as between the rural and urban population have declined and are now relatively small (Demirgüç-Kunt et al., 2018). Financial inclusion in our sample is similar: 78.34% of our sample households have an account with a formal banking institution.

Simultaneously, the rural credit market has evolved extensively, providing manifold loan options for consumers. This is mainly due to heavily subsidized government programs. The market is dominated by government-financed institutions (Chichaibelu and Waibel, 2017). The most important ones are the Bank for Agriculture and Agricultural Cooperatives (BAAC) and the Village and Urban Community Fund (VF) program,[6] with the former reaching approximately 95% of all farm households (Terada and Vandenberg, 2014). This massive expansion can also be observed in our sample, where the majority (73.4%) of households has a loan that is either still owed or has been paid back within the last 12 months. Figure 5.2 provides a graphic overview of the loan situation. Conditional on having a loan, households have on average 2.4 loans. Households borrow from formal and informal sources alike. In fact, loan sources are diverse, with the two most important credit sources being the BAAC and the VF. This lending pattern is similar across all districts we consider. Households also borrow from other sources, for example, from agricultural cooperatives, business partners, money lenders, relatives, and friends. Loans are taken out for various reasons. Most loans are primarily used for agricultural related goods like fertilizer or pesticides (23.96%), for consumption goods (22.39%), and for agricultural investments, e.g.

[5] Still, as a robustness check, we re-run the analysis without respondents who are not at all in charge of financial decision-making within the household.

[6] The aim of the VF is to improve financial access in rural areas in Thailand. It is one of the largest microfinance programs in the world (Menkhoff and Rungruxsirivorn, 2011).

farm land or agricultural machines (16.58%). Loans are also used for paying back another loan (9.87%), buying durable household goods (6.72%), and for education (3.15%).

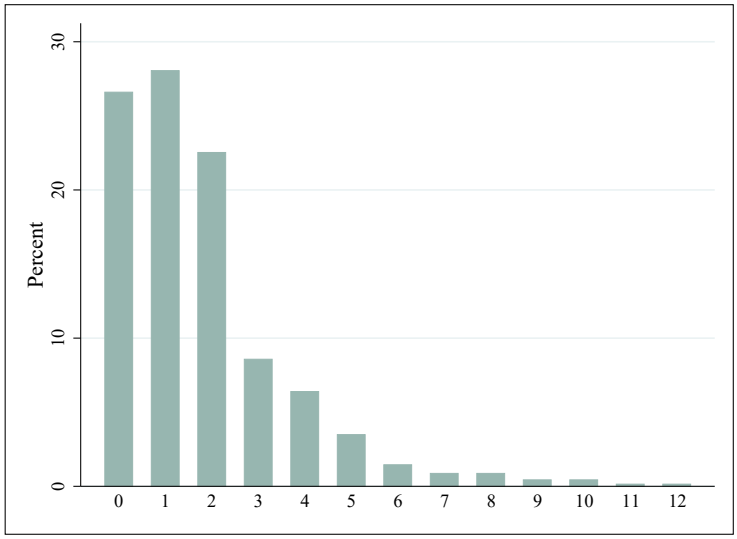


Figure 5.2: Number of Loans

5.2.3 Income Expectations

Households can form positive or negative income expectations. We are interested in studying households that exhibit high (positive) income expectations. In order to obtain a positive income expectation measure, we must elicit income expectations in the first place. Expectations play a central role in the economic theory of household decision-making, for example, with respect to determining saving, borrowing, and consumption (Friedman, 1957), or with respect to occupation choices (Becker, 1964). Manifold research has tried to predict this choice behavior based on expectations. Yet, expectations are challenging to elicit empirically.

Eliciting Income Expectations

Expectations from Former Income Realizations The traditional way of elicitation - referred to as revealed preference analysis - assumes that individuals have *rational expectations* (Dominitz and Manski, 1997; Manski, 2004) and infers expectations from data on past income realizations. For this approach, strong assumptions on the expectation formations process are needed, with both the researcher and the respondent needing to have the same information set (Guiso et al., 2002). Given these strong assumptions and our conjecture that mistakes

in expectation formation are likely to occur in our setting, we decide for two alternative elicitation methods, which are explained in what follows.

Qualitative Expectations Questions The first way is to elicit expectations via qualitative questions, e.g. using Likert scales for questions on future expected events. We use this method in the appendix to replicate the results of Hyytinen and Putkuri (2018), who use Likert scales to elicit financial expectations. However, this approach suffers from two main drawbacks: First, answers might not be comparable across respondents and, second, response options may be too coarse and leave room for responses different from what is proposed.

Subjective Probabilistic Income Expectations Dominitz and Manski (1997) suggest to elicit *probabilistic expectations*. This approach is particularly useful for calculating individual cumulative distribution functions and moments of the relevant variable (Attanasio, 2009). By allowing researchers to retrieve different moments of the expected income distribution, it becomes possible to algebraically study the internal consistency of elicited expectations (e.g. apply the laws of probability) and to use these probabilistic expectations as actual probabilities describing how respondents assess future outcomes. We use this approach in our main analysis to retrieve positive expectations.

As we elicit expectations within a rural sample in an emerging economy, we rephrase percent change questions in a way similar to "how sure are you" and use visual aids to make the concept of probability more comprehensible.[7] Thereby, we address the concerns of Attanasio (2009) and Delavande et al. (2011), who state that the concept of probability might be hard to convey in contexts where people have low levels of education.[8]

To check whether respondents adhere to the basic laws of probability, we first ask them how sure they are that it will rain tomorrow and how sure they are that it will rain within the next two weeks. They can indicate their answer by putting between zero and ten marbles that we gave them beforehand into a cup, with zero marbles meaning they are absolutely sure it will not rain and ten marbles meaning they are absolutely sure it will rain. There are 182 out of 748

[7] Studies dealing with these kind of expectation elicitation include, among others, Attanasio and Augsburg (2016), who study income processes in India, McKenzie et al. (2013), who investigate income expectations of Tongans, and Attanasio and Kaufmann (2014), who elicit income expectations among high school students in Mexico.

[8] The average respondent in our sample only attended school for six years.

respondents (24.33 %) who do not obey the laws of probability: they set a zero chance that it will rain within the next two weeks but a positive probability that it will rain tomorrow. This is a substantial share of respondents, most likely caused by the low educational level in our sample. In the subsequent analysis, we run our regression both with and without these individuals.

After this "warm-up" exercise, we ask respondents how sure they are that their monthly household income in the next twelve months will be in a predefined range. We use income quartiles from the 2013 TVSEP wave to predetermine the four bins to which respondents allocate their ten marbles. The four bins range between 0 - 3,300 Thai Baht (THB), 3,300 - 8,100 THB, 8,100 - 16,590 THB, and 16,590 - 921,000 THB.[9] Respondents distribute their ten marbles based on how likely they think it is that their future monthly income will lie in each specific bin.[10] Hence, we are able to calculate the individual cumulative distribution function (CDF) for the expected monthly income as we interpret the number of marbles distributed between the cups as points on their individual CDFs.

We then fit a subjective income distribution following Attanasio and Augsburg (2016) and assume a piecewise (i.e. per cup) uniform probability distribution. This enables us to calculate a specific expected mean and median income, as well as the standard deviation, for each household.

Table 5.1: Probabilities Assigned to Sections of the Income Distribution

	Observations	Minimum	Maximum	Median	Mean	S.D.
0-3300 THB	737	0	100	20	32.18	35.1
3301-8100 THB	737	0	100	30	30.71	29.27
8101-16590	737	0	100	20	24.03	28.38
16591-300000	737	0	100	0	13.08	24.08

Respondents allocate the number of marbles to the cups as a function of their underlying subjective probability to earn income in the specific income range. The average distribution of marbles per cup, i.e. the average implied probabilities to earn income in the respective income quartile is shown in Table 5.1. Additionally, Figure 5.3 presents the probability density function of expected income in our

[9] The range of the last bin is very broad. Compared to the maximum monthly income respondents state, we find that only two respondents expect an income as high as 921,000 THB. All other maximum income guesses range between 0 - 300,000 THB. In order to avoid artificially high expected median incomes, we restrict the range of the last bin in our calculation of expected median income to a maximum of 300,000 THB.

[10] The enumerator places four cups in front of them, each labelled with a different income range and makes sure that all marbles are allocated at the end of the exercise.

sample. The average respondent's expected income distribution is skewed to the right; that is, on average, respondents believe it is more probable that their average monthly future income is in the lower cups.

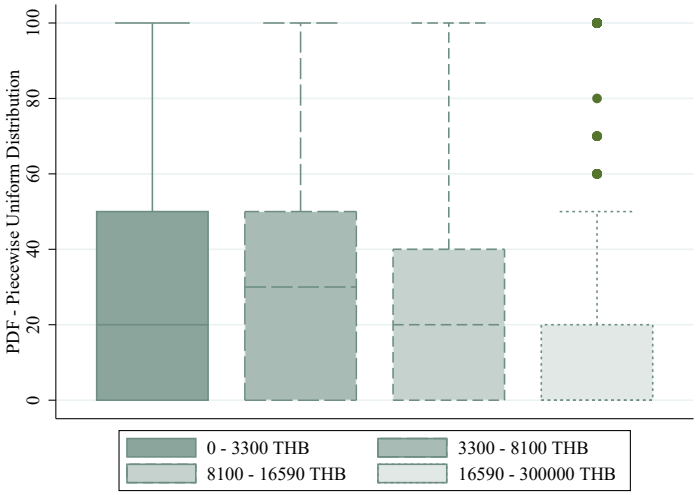


Figure 5.3: Probability Density Function of Expected Income

We also ensure that the elicited expected income is not completely at odds with the actual income process. As measure for the income process, we use the realized income in 2016 and a measure averaging the self-reported income in a very bad and a very good month. Correlations between these and our expected income measure are always statistically significant and range between 0.27 and 0.33, which is encouragingly high given that the correlation between actual income in 2016 and 2017 is 0.48. Furthermore, as Attanasio (2009) proposes, we check how the subjective expected median income covaries with household characteristics, particularly with the composition, education, and realized income (results available upon request). Beyond the already stated relationship with income, household total education is significantly, positively related to the expected median income. A little ambiguous is the correlation to household composition: While a larger number of elders in the household is associated with lower expected income (albeit not significantly), more workers in the household also seem to decrease it.[11]

[11] Reflecting on this last result, we assume that households with more working members are, in general, poorer and have less stable incomes. There is a tendency in Thailand to abolish multi-generational households for small family homes, which is, however, only possible if income is high enough and stable.

Defining Positive Income Expectations

We develop a new kind of positive income expectation measure that is based on the expected future monthly income and the current income. To derive a *quantitative income forecast (Quant. IF)*, we first calculate the percentage change between actual monthly income generated in t and future expected monthly income in $t+1$, which is elicited by the procedure explained in this Section. Specifically, t refers to the year 2017, for which we have actual income data. Consequently, $t+1$ considers income expectations for 2018.

$$Quantitative\,Income\,Forecast\,(Quant.IF) = \frac{E_t(Inc_{i,t+1}) - Inc_{i,t}}{Inc_{i,t}} \times 100 \quad (5.1)$$

In a second step, we divide the quantitative income forecast into quintiles such that our outcome measure allows for five categories ranging from a very negative, negative, mildly negative income forecast, via a neutral income forecast to a positive quantitative income forecast. Thus, the negative (positive) forecasts capture households that expect relatively less (more) future monthly income as compared to their actual earned income in the current year. Each quintile enters the regression via a dummy variable where households with a mildly negative quantitative income forecast (i.e. respondents that range in the third quintile) serve as the omitted group.

In general, respondents are rather pessimistic with regard to their future income. The distribution of changes in expected future income ranges from -98.6% to 19528.6% whereas the maximum is a clear outlier, which also drives the average increase of expected future income of about 35%. If we exclude this household the average shrinks to 6.9%.[12] The median household expects a 51% decrease of future income relative to actual income. Thus, the distribution is skewed to the right. In total, 75% of the sample expect their future income to be lower than the one in the year of the survey. This explains why three of the quintiles clearly range in the negative scope of the distribution and are thus coined "negative income forecast." Only the highest quintile is composed of households that have a clearly positive outlook.[13] The negative outlook on future income may be explained by two developments: First, respondents may fear further political turmoil following the 2014 military coup. Second, the negative outlook may be

[12] The corresponding respondent has a very low income in 2017, but - in the cup game - used all ten balls for the highest income range. We suspect the respondent had not fully grasped the elicitation game.

[13] Variables that covary with each respective forecast group can be found in the Online Appendix.

due to the persistent, regional, economic inequality. People from north eastern Thailand still earn substantially less than people from other regions and, thus, might feel disadvantaged throughout (Lao et al., 2019). According to the World Bank, inequality in Thailand has increased between 2015 and 2017, despite overall economic growth in the country (World Bank, 2019).

While we cannot formally test accuracy of expectations with our subjective expected income data,[14] we assume that a high and positive relative difference between expected income in 2018 and realized income in 2017 is partly due to respondents being too optimistic regarding what they will earn in the future. This assumption is based on studies finding that expectations about various future outcomes may tend toward being positively biased (see for example Zinman, 2015). Furthermore, considering the median household's negative expectation on future monthly income, we are confident that we capture very optimistic households with regard to income development in the highest quintile of the distribution.

We also account for perceived income uncertainty in our analysis. In addition to asking respondents how they think that their income will develop over the next 12 months, we ask how certain they are that this income development will truly become reality. Being potentially too certain about future realizations of stochastic processes can be a form of biased expectation called "overprecision" (Moore and Healy, 2008).

Figure 5.4 provides a graphic overview of the results on our measure for perceived income certainty:

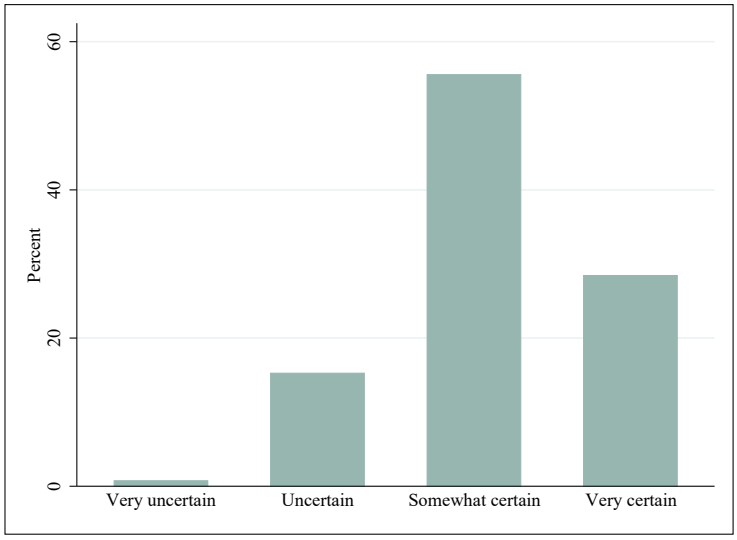


Figure 5.4: Income Certainty

[14] For example, because we lack data about realized income in 2018, the year after we asked for expected income, and we do not know (yet) about shocks households endured during that time.

55.56% of respondents are at least somewhat certain about their income development and 28.44% are very certain. The survey took place during the harvest season, so that respondents might have an idea about the harvest outcome and, therefore, perceive their expected future income as rather certain or they truly suffer from overprecision.

Last, we derive a measure of expectation accuracy following Souleles (2004) and Hyytinen and Putkuri (2018). It is based on a coarser assessment of a household's future income. We can actually determine its accuracy, which is why we call this measure the *qualitative forecast error*. The derivation and estimation results are found in Appendix D.2.

5.2.4 Over-Indebtedness Indicators

There is no consensus regarding a single set of indicators measuring indebtedness precisely, even less so for over-indebtedness.[15] In general, all measures share economic, social, temporal, and psychological dimensions such as that the amount of debt exceeds income over a medium- to long-term time horizon and the household is not able to fulfill its debt commitments without increasing its income or lowering its standard of living, which might lead to stress and worry (D'Alessio and Iezzi, 2013). Furthermore, so-called objective debt measures relate to the household's debt service capacity, subjective measures rather emphasize the psychological consequences of being indebted (Keese, 2012).

Based on the existing literature, we decide to construct two measures of over-indebtedness. The first index captures different dimensions of being "objectively" over-indebted (based on best practices from the literature) while the second index rather refers to "subjectively" felt factors related to financial distress.

Objective Over-Indebtedness Index The objective over-indebtedness measure is an aggregated and standardized index that combines four indicators. We include the following components in the index: an indicator variable if the debt service to income ratio (DSR) is greater than 0.4, an indicator variable if the overall remaining debt service to income ratio exceeds 0.4, an indicator for if the household holds more than two loans at the same time, and one indicator for if the household paid late or defaulted on a loan in the last 12 months. Each component is well established in the literature (see, for example D'Alessio and Iezzi, 2013). Among these variables, the DSR is widely recognized as standard measure to capture indebtedness. The threshold we set for the DSR to indicate

[15] Among others, D'Alessio and Iezzi (2013) provide a summary on different indebtedness indicators, their usage, and possible drawbacks.

over-indebtedness is based on considerations from the literature where a range between 0.3 and 0.5 is used (Chichaibelu and Waibel, 2017; D'Alessio and Iezzi, 2013). In constructing the objective over-indebtedness index we follow Kling et al. (2007). We explain how the index and its components are derived in the Online Appendix. When deriving our debt measures, we include all types of loans that households report. Those can be formal or informal loans, as well as loans taken from friends and family members. During the interview, respondents were highly encouraged to report all loans regardless of the source. Hence, we are confident that we capture a household's true debt level.

Subjective Over-Indebtedness Index While objective debt indicators provide numerically accurate debt measures, they are sometimes criticized for failing to account either for the reasons why households overborrow or for the household's undisclosed ability to pay back debt. Therefore, we also include subjective, "respondent driven" over-indebtedness measures in our analysis. As before, we derive a standardized index aggregating different indicators of subjective over-indebtedness. The indicators include an assessment identifying if the household feels it has too much debt, if it has difficulties paying debt off, and the so-called "sacrifice index."[16] The index and its components are explained in detail in the Online Appendix. Schicks (2013) prefers to use subjective over objective debt measures in her work analyzing over-indebtedness from a customer-protection point of view in microfinance. D'Alessio and Iezzi (2013) also rely heavily on a subjective measure to study over-indebtedness in Italy. In line with Keese (2012) and Lusardi and Tufano (2015), we argue that subjective measures describe a situation of financial distress for the respective households but are, naturally, highly subjective to the respondent such that these measures should not be used without considering objective indicators as well. For all indices derived, higher scores point at a higher value of accumulated debt.

Table 5.2 depicts the summary statistics of the objective and subjective over-indebtedness indices. The objective index ranges from -1 to 3 with higher values indicating a more severe level of over-indebtedness. While the average DSR lies at 0.23, about 18% of the households have a DSR that is higher than 0.4. More strikingly, about 23% of our sample households have more than two loans. The range of the subjective index is between -2 and 3, again oriented in a way that higher numbers point to higher indebtedness. On average, households state that they have the "right amount of debt" (Mean = -0.02 for the debt position variable) and that they have no difficulties paying off debt. However, the average household

[16] We closely follow Schicks (2013) in constructing the sacrifice index.

admits to have made at least some sacrifices regarding household needs due to lack of money as the average value is -0.08 and a household with no sacrifices would be found at the lowest end of the sacrifice index distribution.

Furthermore, Table D.1.1 presents correlations between all our debt indicators. Naturally, the objective and subjective indices are significantly correlated with their respective sub-indicators. However, our objective and subjective measures also correlate significantly with each other. This is encouraging, since it rebuts criticism with respect to objective over-indebtedness measures neglecting important dimensions of financial distress.

Table 5.2: Summary Statistics - Over-Indebtedness Variables

	Mean	S.D.	Min	Max	Observ.
Objective Index	0.00	0.99	-1	3	688
DSR > 0.4 (=1)	0.18	0.39	0	1	688
Holds > 2 Loans (=1)	0.23	0.42	0	1	688
RDSR > 0.4 (=1)	0.40	0.49	0	1	688
Paid Late/Default (=1)	0.15	0.36	0	1	685
Subjective Index	-0.02	0.98	-1	4	688
Debt Position	-0.02	0.86	-1	1	688
Diff. Paying Debt (=1)	0.06	0.25	0	1	686
Sacrifice Index	-0.08	1.19	-2	4	688

Note: The debt index variables are standardized. The components of the indices are given in non-standardized real terms.

5.3 Survey Results

In the following, we relate the quantitative income forecast to the over-indebtedness indices by running OLS regressions, estimating correlations between the respective variables.

5.3.1 Estimation Strategy

The regressions we run take the following form:

$$Over\text{-}Indebtedness\ Index_i = \beta_0 + \beta_1 Quant.\ IF_i + X_i^{'}\beta_2 + \epsilon_i \tag{5.2}$$

The dependent variable $Over\text{-}Indebtedness\ Index_i$ represents the debt measures we apply to mirror financial distress of the household. It contains either the

objective over-indebtedness index,[17] or the subjective over-indebtedness index.[18] The main variables of interests are captured in $Quant.\,IF_i$. It comprises the income forecast groups (quantitative income forecast) we derived in Section 5.2.3, where the mildly negative forecast group serves as reference group. We cluster our standard errors at the district level.[19]

The vector X_i controls for household and respondent characteristics that are likely to influence household over-indebtedness: dummies for farming, self-employment, and wage employment, monthly household income in 2016 and 2017, the number of children between the age of 0-6, 7-10, and 11-16 years, the number of elders and working members, total household education (sum of all educational levels in the hh), age and age squared of the respondent, and respondent's financial literacy score. The vector also captures the monetary loss from past shocks. We use detailed information from 2016 and 2017 about monetary losses directly related to a shock. We differentiate between losses from farming related shocks, environmental shocks, economic shocks, crime shocks, and other shocks.

5.3.2 Main Results

To begin with, we relate the quantitative income forecast groups to each over-indebtedness index (OI-Index). Then, we add the aforementioned control variables to our regression as the indices depend on other respondent and household specific characteristics as well. Tables 5.3 and 5.4 provide results for the objective and subjective OI-Indices. The tables show results for the four income forecast groups as well as for the shock loss control variables (tables including results for all covariates are presented in the Online Appendix). The first column in each table represents the standardized and averaged index whereas the subsequent columns depict results for the single non-standardized components of the indices.

Objective Over-Indebtedness We find a strong, statistically significant, relationship between positive income forecasts and the objective OI-Index. Households with high future income expectations compared to their actual income are

[17] Standardized average of a dummy equaling one if the debt service to income ratio is greater than 0.4, a dummy equaling one if the remaining debt to income ratio is greater than 0.4, a dummy regarding whether the household paid late or defaulted on a loan, and a dummy equaling one if the household has more than two loans.

[18] Standardized average of the sacrifice index, answers to questions on debt position and whether the household has difficulties paying off debt.

[19] Cameron and Miller (2015) advise to cluster at least at the primary sampling unit, which is the district level in our case. Since this gives us a small number of clusters, as a robustness check, we use wild cluster bootstrap. This does not change our main findings.

more likely to be over-indebted. The over-indebtedness index increases by 0.29 - 0.31 points for positive income expectations (columns (1) and (2), Table 5.3).

Table 5.3: Objective Over-Indebtedness

	Obj. Index		DSR > 0.4		RDSR > 0.4		Paid Late/Default		> 2 Loans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Very Negative	−0.125	−0.017	−0.097*	−0.022	−0.073	0.011	0.017	−0.015	0.001	0.010
	(0.151)	(0.143)	(0.047)	(0.050)	(0.081)	(0.079)	(0.033)	(0.036)	(0.059)	(0.060)
Negative	0.050	0.058	−0.067	−0.054	0.075	0.100*	0.081**	0.066**	−0.029	−0.037
	(0.134)	(0.132)	(0.045)	(0.048)	(0.058)	(0.057)	(0.032)	(0.029)	(0.057)	(0.058)
Neutral	0.153	0.135	0.025	0.002	0.079	0.067	0.074	0.095*	−0.002	−0.010
	(0.153)	(0.168)	(0.050)	(0.060)	(0.058)	(0.064)	(0.045)	(0.051)	(0.061)	(0.063)
Positive	0.289**	0.333**	0.098**	0.087*	0.187**	0.210***	0.109***	0.133***	−0.054	−0.037
	(0.134)	(0.136)	(0.042)	(0.047)	(0.072)	(0.069)	(0.038)	(0.041)	(0.055)	(0.060)
Farm. Shocks		−0.000		−0.000		0.000		−0.000		0.000
		(0.002)		(0.000)		(0.001)		(0.001)		(0.001)
Env. Shocks		0.005***		−0.000		0.002***		0.002**		0.002***
		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)
Econ. Shocks		0.003***		0.000		0.002***		0.001*		0.000
		(0.001)		(0.000)		(0.001)		(0.001)		(0.001)
Crime Shocks		−0.016*		−0.004*		−0.013***		−0.002		−0.001
		(0.009)		(0.002)		(0.003)		(0.004)		(0.004)
Other Shocks		−0.000		−0.000		−0.000		0.000**		−0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Constant	−0.073	−1.425**	0.189***	0.119	0.343***	−0.617**	0.099***	−0.016	0.245***	−0.291
	(0.144)	(0.576)	(0.048)	(0.296)	(0.072)	(0.286)	(0.019)	(0.243)	(0.063)	(0.280)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	688	676	688	676	688	676	685	673	688	676
Adj. R-squ.	0.014	0.099	0.025	0.046	0.025	0.125	0.007	0.044	-0.003	0.053

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered SE in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

This relationship is mainly driven by the remaining debt ratio and the dummy on if the household paid late or defaulted on a loan. The debt service to income ratio is only marginally significantly related to positive expectations and having more than two loans shows no relation at all. The RDSR increases by 18.7 - 20.7 percentage points (columns (5) and (6)) and the probability that a household paid

late or defaulted on a loan increases by 10.9 - 12.4 percentage points for households whose expected future median income is greater than the current income (columns (7) and (8)). Furthermore, the coefficient of the dummy indicating a DSR greater than 0.4 increases by 8.4 - 9.8 percentage points (columns (3) and (4)) for those households.

With regard to the other forecast groups, we do not find consistent results. While the probability of a household defaulting or paying late slightly increases for households with a negative forecast, overall, results for the non-positive groups are insignificant, if not showing a negative sign. A significant and robust link to over-indebtedness can only be found for households with positive expectations. We account for monetary losses from various shock events, because a shock might influence both the level of over-indebtedness and income expectations at the same time (i.e. an expectation to return to pre-shock-level income). The results show that higher losses are associated with higher debt levels. However, while we find statistically significant effects, these effects are economically rather small. For example, if an environmental shock loss increases by 1000 Thai Baht (ca. 26€ in 2017), the objective OI-Index increases by 0.05 points. Even when accounting for monetary losses induced by shocks, the relationship between positive forecasts and over-indebtedness remains significant, confirming a robust relationship.

Concerning additional covariates, household income and the perceived social status are significantly negatively related to household over-indebtedness. Age is positively and age squared negatively significant, suggesting a hump-shaped pattern in line with life-cycle-income-smoothing. Furthermore, over-indebtedness remains largely unaffected by household composition and education.

Subjective Over-Indebtedness Our analysis of subjective over-indebtedness reveals that the relationship to the positive income forecast group is less pronounced than for the objective over-indebtedness index but still significant for the index and all its components. As shown in Appendix D.2, the qualitative forecast error is more strongly related to the subjective OI-Index. This hints at two possible explanations: One, the subjective OI-Index is rather a concept of perceived financial distress and, thus, more related to the “more subjective" qualitative forecast error. Two, financial distress is not only determined by the household’s true debt situation but more so by its perception. When analyzing the control variables, we find that risk seeking and the perceived social status of the household are highly significantly related to the subjective OI-Index, much more so than other control variables.

Table 5.4: Subjective Over-Indebtedness

	Subj. Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Very Negative	0.182	0.215*	0.040	0.036	0.065**	0.058	0.118	0.245**
	(0.112)	(0.122)	(0.114)	(0.110)	(0.029)	(0.039)	(0.106)	(0.103)
Negative	0.157	0.150	0.096	0.046	0.037	0.033	0.108	0.178
	(0.135)	(0.110)	(0.111)	(0.109)	(0.025)	(0.026)	(0.174)	(0.154)
Neutral	−0.007	0.048	−0.021	0.008	0.022	0.031	−0.098	−0.035
	(0.104)	(0.092)	(0.096)	(0.094)	(0.021)	(0.019)	(0.128)	(0.095)
Positive	0.144	0.258**	0.113	0.181**	0.024	0.041*	0.113	0.245*
	(0.086)	(0.101)	(0.071)	(0.084)	(0.021)	(0.023)	(0.120)	(0.122)
Farming Shocks		−0.001		0.002		−0.000*		−0.002
		(0.001)		(0.001)		(0.000)		(0.002)
Environmental Shocks		0.007***		0.003***		0.002**		0.003
		(0.001)		(0.001)		(0.001)		(0.002)
Economic Shocks		0.001		0.003**		−0.000		−0.000
		(0.001)		(0.001)		(0.000)		(0.002)
Crime Shocks		0.000		−0.006		0.003		−0.005
		(0.014)		(0.007)		(0.003)		(0.014)
Other Shocks		0.002***		0.000		0.001***		0.002***
		(0.001)		(0.000)		(0.000)		(0.000)
Constant	−0.115	−0.482	−0.064	−1.480***	0.035**	0.140	−0.131	0.344
	(0.082)	(0.593)	(0.081)	(0.514)	(0.016)	(0.155)	(0.111)	(0.591)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	688	676	688	676	686	674	688	676
Adj. R-squared	0.001	0.133	-0.002	0.094	0.002	0.073	-0.001	0.119

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered SE in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6, 7-10, and 11-16), financial literacy score, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Delving deeper into respondent characteristics, we run regressions including the Big Five measures,[20] (tables are presented in the Online Appendix). For respondents who score high on openness and neuroticism, the subjective OI-Index and its components are larger than for those who score low. Eventually, shocks are similarly related to subjective over-indebtedness as they are to objec-

[20] The Big Five comprise five personality traits: openness, conscientiousness, extraversion, agreeableness, and neuroticism. Details on their construction are in the Online Appendix.

tive over-indebtedness: Households experiencing an environmental shock have a significantly higher perceived debt level.

Income Certainty Next, we investigate whether being potentially too certain about the future income development is related to over-indebtedness. As shown in Tables 5.5 and 5.6, there is no relation between certainty about future income and subjective over-indebtedness, although we find that higher income certainty is related to objective over-indebtedness. If a respondent is very certain about the development of future household income, this is linked to an augmented over-indebtedness index. This result is mainly driven by the debt to service ratio and by having more than two loans (columns (2) and (5), Table 5.5).

Table 5.5: Certainty Measure - Objective Over-Indebtedness

	Obj. Index	DSR > 0.4	RDSR > 0.4	Paid Late	> 2 Loans
Very Negative	−0.017	−0.023	0.012	−0.017	0.013
	(0.144)	(0.050)	(0.079)	(0.036)	(0.061)
Negative	0.047	−0.062	0.104*	0.057*	−0.034
	(0.129)	(0.044)	(0.054)	(0.030)	(0.057)
Neutral	0.122	−0.002	0.062	0.092*	−0.013
	(0.167)	(0.060)	(0.064)	(0.051)	(0.063)
Positive	0.323**	0.084	0.201***	0.131***	−0.037
	(0.140)	(0.051)	(0.070)	(0.043)	(0.061)
Certainty	0.129**	0.052**	0.046*	−0.008	0.061**
	(0.061)	(0.022)	(0.026)	(0.024)	(0.022)
Constant	−1.564**	0.074	−0.705**	0.064	−0.413
	(0.552)	(0.299)	(0.284)	(0.268)	(0.276)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	664	664	664	661	664
Adj. R-squared	0.101	0.054	0.125	0.042	0.060

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered SE in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table 5.6: Certainty Measure - Subjective Over-Indebtedness

	Subj. Index	Debt Position	Diff. Pay off Debt	Sacrifice Index
Very Negative	0.220	0.049	0.057	0.247**
	(0.133)	(0.117)	(0.041)	(0.108)
Negative	0.144	0.045	0.032	0.168
	(0.109)	(0.108)	(0.026)	(0.150)
Neutral	0.043	0.010	0.030	−0.048
	(0.092)	(0.095)	(0.019)	(0.097)
Positive	0.238**	0.177*	0.035	0.227*
	(0.110)	(0.098)	(0.023)	(0.125)
Certainty	0.069	0.092	0.006	0.031
	(0.086)	(0.066)	(0.020)	(0.104)
Constant	−0.673	−1.802***	0.143	0.273
	(0.651)	(0.578)	(0.165)	(0.699)
Controls	Yes	Yes	Yes	Yes
Observations	664	664	662	664
Adj. R-squared	0.133	0.098	0.072	0.115

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered SE in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Overall, we conclude, (i) that there is indeed a significant positive and robust relationship between positive quantitative income forecasts and objective as well as subjective over-indebtedness; (ii) We are also reassured that, although correlated to each other, subjective and objective over-indebtedness indicators measure different dimensions of indebtedness. The "hard" objective OI-Index is much stronger related to positive income forecasts than the subjective OI-Index; (iii) Certainty about the household's income development is also related to over-indebtedness, primarily to objective over-indebtedness.

5.3.3 Robustness

Excluding Possibly Confounding Observations. Before eliciting the subjective expected income of respondents, we ask two questions testing the understanding

of the concept of probability. We re-run the analysis including only those respondents who do not violate the laws of probability and examine whether our main results hold. Results are presented in Tables D.1.2 and D.1.3 in the Appendix. The coefficients for this sub-sample stay highly significant and almost all coefficients increase in size emphasizing the link between a positive income forecast and objective over-indebtedness. In order to verify that respondents have an actual understanding of their household's finances, we again re-run the regressions, including only those individuals who are in charge of the household's financial decisions either alone or together with someone else (see Appendix Tables D.1.4 and D.1.5). Overall, the results stay virtually unchanged with regard to the significance of our coefficients of interest. Point estimates change slightly.

Interacting the Income Forecast with Personality Traits. We do not claim to show a causal effect because - among other reasons - we acknowledge that the relation between over-indebtedness and positive income expectations may also work in the reverse. For example, if people are indebted, they might have a great bias regarding future expected income as they plan to work harder in the future to pay down their debt. We expect such people to exhibit a high level of conscientiousness, the personality marker describing achievement oriented (McClelland et al., 1953), hard-working, effective, and dutiful characters (Barrick and Mount, 1991). Hence, we interact our income forecast measure with this character trait, expecting to find significant effects for conscientious people. Results for the aggregated indices as dependent variables are presented in Appendix Table D.1.6. The interaction is not significant for the positive income forecast and any of the OI-Indices. This counteracts the assumption that the achieving respondents with distorted expectations drive the relationship between our positive income forecast and debt status.

Exchanging the Forecast Groups with One Single Indicator. We apply a coarser indicator measuring positive future income expectations to counteract the possible criticism that our results hinge on the choice of the reference category with respect to our income forecast groups. In lieu of the five quantitative income forecast groups, we define an indicator variable to turn one if the relative difference between expected future and actual income is greater than zero. Results for the objective and subjective over-indebtedness indices as well as for the certainty measure are presented in Appendix Tables D.1.7, D.1.8, D.1.9, and D.1.10. Probably due to the broader category that we use as the main explanatory variable,

point estimates gain in significance, but are numerically a little smaller when compared to the positive income forecast group. This actually supports our finding that it is exactly those respondents with high expectations about their future income who are also relatively more indebted. Generally, this robustness check confirms that our results remain significant and similar in size with respect to the objective and subjective over-indebtedness indicators when using a broader income expectation indicator. Hence, it is not the choice of the reference group that drives our results.[21]

5.4 The Experiment

The preceding section shows that high expectations and over-indebtedness are strongly related to each other in our rural Thai population, even when controlling for important socio-economic characteristics and shocks. However, methodologically, the implemented regression analysis only represents correlations. Furthermore, we are specifically interested whether overconfidence, a systematic behavioral bias that might be responsible for having too high expectations in the first place, can actually cause overspending and overborrowing. In what follows, we analyze if overconfidence is one potential *cause* why households in our sample spend more than they can actually afford.

Theoretically, upward biased expectations can arise for two reasons; either an individual is overly optimistic or overly confident. We follow Heger and Papageorge (2018) in defining overoptimism as the tendency to overestimate the probability of preferred outcomes and overconfidence as the tendency to overestimate one's own performance. We acknowledge that in our rural, agricultural setting, overoptimism might occur as frequently if not more than overconfidence. Since agricultural activities and the exposure to weather shocks are rather homogeneous in our sample and less driven by personal abilities, a more positive view on the future might originate from an optimistic view on the world in general. Still, there is scope for overconfidence as the adoption of new agricultural technologies and crops, the working pace (that can influence agricultural output) and the bargaining power in selling crops is strongly dependent on beliefs about individual performance and might lead to positive income expectations as well. For our experiment, we concentrate on overconfidence because numerous studies

[21] Additionally, we also used different reference groups in the first place and our regression results remain similar. Results are available upon request.

show that overconfidence is related to important life and financial decisions, while overoptimism is less so.[22]

5.4.1 Experimental Design

As final part of the survey, we play a "market game" in which respondents can buy different kinds of goods for a discounted price with money they earn in the experiment. They can buy packs of coffee, chips, dried mango, or detergent for 10 THB (ca. 0.25€) each instead of the 20 THB list price.[23] Each participant receives an endowment of 40 THB. Additional money can be earned by answering questions in a trivia game. Earnings depend on how many questions the participant answers correctly in comparison to the other participants. We rank them from 1-10, where rank ten corresponds to answering the most questions correctly and rank one to answering the least number of questions correctly.[24] People ranked 1-4 do not earn anything on top of their endowment, those ranked 5-6 earn 10 THB, those ranked 7-8 20 THB, and those ranked 9-10 earn 40 THB additionally. Thus, participants can earn up to 80 THB and can buy at most eight goods.

We make expectations a crucial factor in the game by requiring participants to decide how much and what to buy before they take the pay-off relevant quiz, i.e. before they know their final payoff. We divide participants in two treatment groups; one group faces a "hard" quiz and the other one an "easy" trivia quiz. To convey the difficulty of each quiz and to exogenously vary expectations about relative performance, participants do a test quiz with seven questions upfront where difficulty again depends on treatment. Based on the test quiz, participants infer

[22] For example, Camerer and Lovallo (1999), who experimentally test the effect of overconfidence on entrepreneurial decision-making (this relationship is a well-researched field of study), conclude that excess entry in a market game is strongly related to overconfidence and not to overoptimism.

[23] At least for the bag of chips, it is common knowledge that they usually cost 20 THB as, for a long time, they had the price printed on their front. To further convince participants that the products are truly discounted, we attached "20 THB" price tags to each product.

[24] In the field, participants from the first villages were ranked against participants from our pilot villages and our interviewers who also took the quizzes. For later villages, we replaced our interviewer data with data from the previous villages and told participants that they are ranked against ten persons who live in a village similar to theirs. For the final analysis, we use all the observations to create a ranking. In each treatment, we have two accumulation points in the number of correctly answered questions that are next to each other and around the mean. We set these two points as rank five and six. Each one point deviation in correctly answered question then constitutes a one point deviation in rank (e.g. if rank five means nine questions answered correctly, rank four means eight questions answered correctly). Since there are more questions than possible ranks, we have some bunching of correctly answered questions around rank one and rank ten, the boundaries of the ranking.

how good they will be in the pay-off relevant main quiz and form expectations about the performance of the others and, thereby, their relative rank. They are ranked within each treatment group and they are told that everybody they are ranked against took the exact the same quiz. With this design, we can exploit the so-called hard-easy gap analogous to Dargnies et al. (2019) and very similar to Grohmann et al. (2019). Much research finds that people tend to overplace themselves in easy tasks and to underplace themselves in hard tasks (for example Merkle and Weber, 2011; Hartwig and Dunlosky, 2014; Benoit et al., 2015). Over-(under-)placing is a form of over-(under-)confidence in which individuals over-(under-)estimate their relative performance in comparison to others. Thus, by assigning participants to two different treatments, we exogenously vary their expectations through varying self-confidence (see Figure 5.5).[25] We subsequently measure confidence as the difference between expected rank and actual rank:

$$confidence = rank_{exp} - rank_{act} \tag{5.3}$$

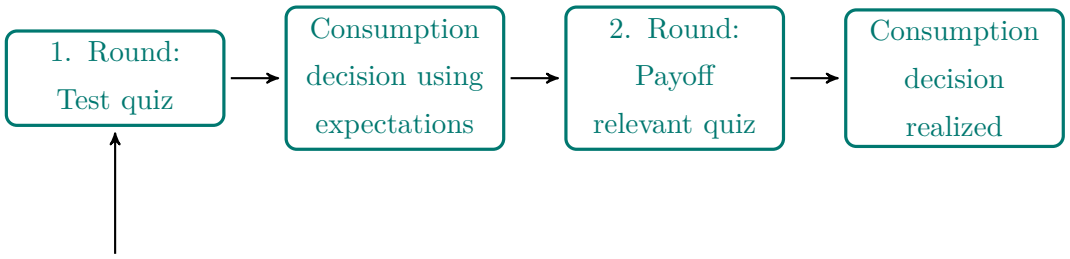


Figure 5.5: Experimental Flow

Except for the difference in difficulty, the procedure is the same for every participant: If participants agree to play the game, the interviewer prepares the set-up and starts reading the instructions. The instructions include comprehension questions to test whether participants understand how their rank is determined and how much they can earn. If participants do not answer these questions correctly, the interviewer does not continue with the instructions.[26] After they have finished the instructions, the participants start answering the test quiz, which has seven trivia questions. They have five minutes to answer all the questions.

[25] The exogenous variation is one reason why we do not include this measure for self-confidence in our survey regressions as a measure for expectation bias. Another reason is that self-confidence is domain dependent.

[26] Still, there are participants who had serious difficulties in understanding the game such that we exclude them from the main analysis

For each question, four possible answers are given. When the time is up or participants have finished answering, they receive a decision sheet. On the decision sheet, they first have to write down the rank and the earnings they expect to reach in the following main quiz. Then, they must indicate their buying decision based on their expected earnings. Afterwards, participants continue with the main quiz where they have to answer 15 questions in ten minutes. Following the quiz, there are three debriefing questions including a question on the expected rank after the second quiz has actually taken place (such that we can check for belief updating). Finally, the interviewer calculates the rank and earnings, then hands over the products and money, if applicable.

In most cases, participants could read, write, and answer the quizzes on their own. Sometimes, people, in particular the elderly, needed assistance in reading and writing, which was provided by the interviewer. The supplemental material for the experiment is found in the Online Appendix in English (for the experiment everything was translated to Thai).

Rational Decisions

If participants want to buy more than they can afford, including their endowment, their consumption has to be restricted. They receive at most as many goods as they can buy with their earnings and nothing beyond that amount. Participants are aware of this fact.

We implicitly assume that expectations influence buying decisions. If this does not hold, the aforementioned design feature seriously distorts our results as follows. If it was the case that "rational" participants strictly prefer goods over money because, for example, they are cheaper than list price and can be stockpiled, expectations would become meaningless for the consumption decision. Indicating to buy eight goods is weakly dominating any other number of goods for this kind of participants, since they clearly prefer goods over money independent of the budget.[27]

Eventually about 4% of our participants decided to buy eight goods even though they expect to earn less. An additional 3% wanted to buy more than they expected to earn but less than eight goods. In our main analysis, these observations are excluded because i) we already know that expectations do not impact consumption in this setting for them and ii) they could artificially inflate

[27] If the participant expects less than 80 THB, there is a potential loss in indicating to buy less than eight goods because the prediction might be underconfident. However, given our setting, there is no loss if she indicates buying eight goods but actual earnings are less than 80 THB.

our results. We present additional analyses on this sub-sample in the Appendix Section "The Rationals" (5.5) and discuss whether they truly acted in a rational way or rather had difficulties understanding the game.

For the other 93%, we still assume that respondents generally prefer a bundle of products and cash. The exact composition depends on individual preferences but also expected earnings. Thus, being overconfident (or underconfident) creates a distortion in utility. Following these reflections, we derive the following hypotheses:

Hypothesis 1: *On average, individuals in the easy treatment will buy more than individuals in the hard treatment.*
Hypothesis 2: *A great level of overconfidence will lead to excessive spending.*

Hypothesis 1 is implied by the finding on the hard-easy gap. Hypothesis 2 follows from the fact that we define respondents to be overconfident if their expected rank is higher than their actual rank, which implies that they earn less than expected. Since we cannot allow respondents to pay from personal money if experimental money is insufficient, restricting consumption in some cases is necessary. Therefore, people cannot accumulate debt. Still, we try to mimic real life financial decision making with this design, especially the fact that sometimes (and optimally) consumption decisions must be made before income is realized. In that sense, participants still have to take a loan, although only for a short time and without serious consequences, if they want to consume. Further, if they have biased beliefs, they might end up with a consumption bundle that is sub-optimal, thus overborrowing. The process can also be seen as a form of household budgeting; however, we prefer the term overborrowing as participants have to plan with money they do not have in the moment of planning. In real life those who overborrow accumulate more debt than optimal, perhaps more than they are able to repay.

5.4.2 Experimental Results

Overall, 604 respondents participated in the game. Since participation is self-selected, participants and non-participants are compared in Table D.3.1 in the Appendix. As can be seen, participants and non-participants differ significantly in some variables.[28] In all these variables, the difference is in the expected direction: female, older, less occupied, less educated, financial illiterate and less numerate,

[28] A complete list of all variables and their explanation is provided in the Online Appendix.

and more financial risk averse respondents are less likely to participate in the game. Several of these variables are significantly correlated with each other. Running a simple regression on the likelihood to participate, we find that some of these variables are insignificant and that the time of day is one of the strongest predictors of game participation (see D.3.2). Since the time of day at which we visited households for the interviews is mostly exogenous,[29] self-selection into the game is less pronounced than initially expected.

Out of the 604, seven observations are excluded because either treatments for them are mixed up, personal information is missing, or a third person helped them answer the questions. We exclude 44 observations that are also excluded from the survey regression analysis because they are outliers in income or the debt service to income ratio (see Section 5.2.1).[30] Additionally, 84 observations are excluded because it can be inferred from the data that comprehension was insufficient[31] or because they want to buy more than they expect to earn in total (see previous Sub-Section on these special cases). Those 84 cases differ only in their number of children between 7-10 years.

In Table 5.7 characteristics of the remaining 471 participants are compared across treatments. The significantly unequal number of participants per treatment is due to fact that we slightly over-sampled the easy treatment. Results from previous studies suggest that the effect of easy tasks on self-confidence is generally stronger than the effect of hard tasks (see for example Dargnies et al., 2019). The characteristics depicted here might be important for the general level of self-confidence and the willingness to buy products. Given the sample size and the number of variables analyzed, randomizing participants into the treatments worked well; the two groups only significantly differ with regard to their health status, their monthly household income, and their (objective) over-indebtedness index. Controlling for these variables leaves our results virtually unchanged and a f-test on joint orthogonality finds that controls do not jointly determine the treatment group.

[29] We interviewed households according to a schedule we designed together with our interview team manager, which tried to minimize travel distances for each interview team. Hence, this schedule was exogenous to individual household characteristics, except for the village that the household resides in. However, a few houses were empty the first time we visited them and we had to reschedule another date with the household itself.

[30] The results are robust to this exclusion.

[31] For example, one participant writes that he expects to earn 30 Baht from the game, which is, however, not an possible option. Another one wants to buy 35 products although the maximum affordable number is eight.

Table 5.7: Descriptive Statistics across Treatments

	Full Sample	Hard Treatment	Easy Treatment	Difference
Sex	1.64	1.60	1.67	−0.07
Age	56.16	55.23	56.93	−1.70
Relation to HH Head	1.70	1.69	1.71	−0.02
Marital Status	2.13	2.09	2.16	−0.07
Main Occupation	4.79	4.29	5.20	−0.90
Years of Schooling	5.92	6.08	5.79	0.28
Children (0-6 years)	0.33	0.37	0.29	0.08
Children (7-10 years)	0.26	0.26	0.26	0.01
Numeracy	2.14	2.09	2.19	−0.10
Health Status	1.38	1.32	1.43	−0.11**
BMI	23.58	23.25	23.86	−0.61
Fin. Decision Maker	1.57	1.55	1.59	−0.03
Self Control	20.94	21.19	20.75	0.44
Risk Taking	4.02	3.96	4.07	−0.12
Fin. Risk Taking	4.06	3.99	4.12	−0.13
FL-Score	5.66	5.55	5.75	−0.20
Monthly Inc. 2017	18653.06	20802.79	16893.44	3909.35**
Obj. OI-Index	0.01	−0.09	0.09	−0.18**
Subj. OI-Index	−0.04	−0.03	−0.06	0.03
Morning	0.53	0.51	0.54	−0.03
Midday	0.27	0.26	0.28	−0.02
Read Alone	1.44	1.44	1.44	−0.00
Difficulties in Game	1.14	1.15	1.13	0.01
Observations	471	212	259	471

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels.

Shift in Beliefs

On average, participants answered 9.07 out of 15 trivia questions correctly in the easy treatment and 5.09 out of 15 in the hard treatment. Thus, it can be assumed that, for our sample, the easy treatment is truly "easier" than the hard treatment. The average expected rank in the hard treatment is 6.89 whereas the average expected rank in the easy treatment is 7.22. In Figure 5.6 the cumulative distribution functions of the expected ranks for both treatments are plotted. It seems that there is only a small shift in beliefs, since the distributions are still almost overlapping.[32] Indeed, if we compare the distributions of the "second" expectations that are elicited after respondents actually took the main quiz, we

[32] We focus on the expected rank in our analysis but everything holds analogously for expected earnings.

find a much larger shift (see Appendix Figure D.3.1). Thus, either our test quizzes are not as hard or easy as the main quizzes and, therefore, the shift in first beliefs is smaller or participants have such strong beliefs that they only gradually update their beliefs. Still, the distributions of first beliefs are significantly different from each other (Kolmogorov-Smirnov one-sided p=0.056; Wilcoxon rank-sum two-sided p=0.041). The t-test for mean expectations is significant at the 5% level (one-sided) as well (Figure 5.9).

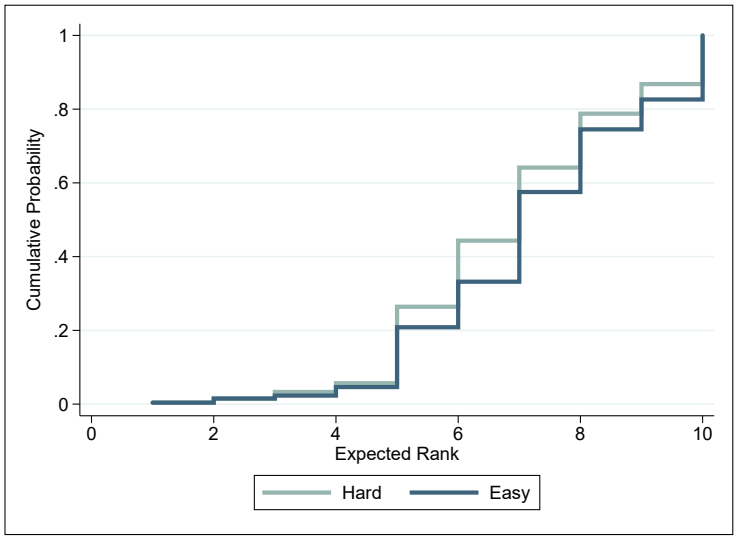


Figure 5.6: Cumulative Density Distribution of Expected Rank by Treatment

The difference in self-confidence is larger than the difference in expected rank (see Figure 5.7). This might be driven by our ranking procedure or by the fact that the easy quiz is not a perfect shift of the hard quiz with respect to the number of questions answered correctly. In any case, this suggests that our manipulation via the treatments to shift the level of beliefs and thereby self-confidence worked.

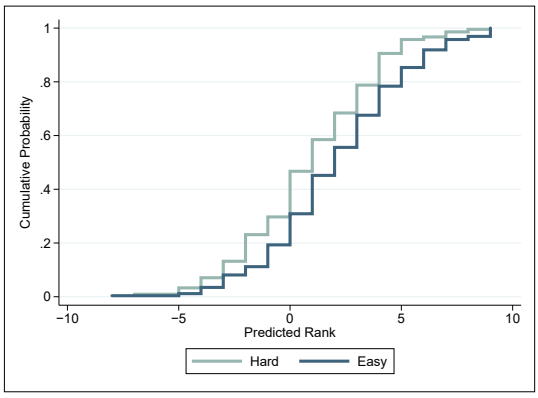


Figure 5.7: CDFs of Self-Confidence

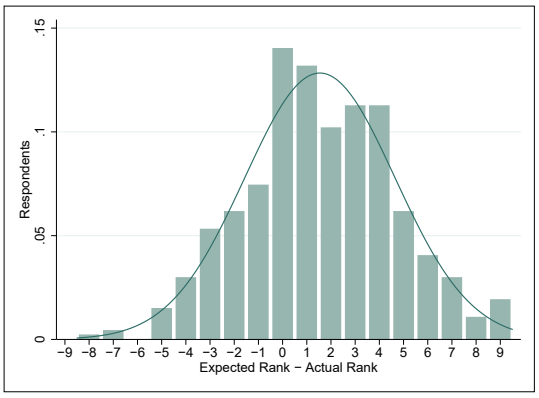


Figure 5.8: Histogram Self-Confidence

As seen in Figure 5.8, across both treatments, the mean and median respondents are slightly overconfident (even in the hard treatment). The whole distribution is a little bit skewed to the left but still resembles a normal distribution. Over 14% of the sample have perfectly accurate beliefs and have a self-confidence of "0." Small deviations from 0 could be considered accurate as well because they could present a form of Bayesian updating.[33] Still, a substantial fraction of participants seems to be tremendously overconfident.

Buying Decision

We find a significant positive correlation between expected rank (earnings) and the number of goods participants want to buy. However, there is no significant relation between the treatment itself and mean desired consumption as presented in Figure 5.9.

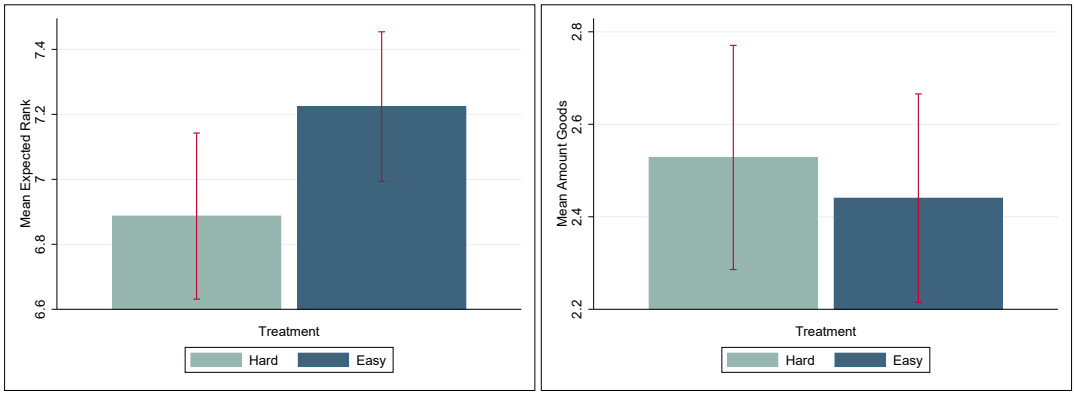


Figure 5.9: Mean Expected Rank (left) and Consumption (right) by Treatment

If we run regressions where we can control for the variables that are unbalanced across treatments, the picture stays the same: the treatment is positively related to the expected rank, the expected rank is positively related to the desired amount of goods, but the treatment is not related to the amount of goods (see Table 5.8).

A similar pattern emerges if we look explicitly at spending behavior (see Table 5.9). We distinguish *overborrowing*, meaning buying more than actual earnings including endowment can pay for, from *overspending*, meaning buying more than actual game earnings can pay for, but the spending can still be paid with the endowment. The expected rank as well as confidence have a significant effect on both variables, but treatment does not.[34]

[33] On this discussion, see Merkle and Weber (2011).

[34] The level of significance is higher not lower when we exclude possibly "rational" participants who want to buy more than they expect to earn in total.

Table 5.8: Consumption Decision

	Exp. Rank	No. Goods		
	(1)	(2)	(3)	(4)
Treatment	0.377**	−0.133		−0.189
	(0.175)	(0.173)		(0.171)
Exp. Rank			0.144***	0.149***
			(0.046)	(0.046)
Controls	Yes	Yes	Yes	Yes
Observations	470	470	470	470

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Robust standard errors in parentheses. Treatment: 0=Hard Quiz, 1=Easy Quiz; A higher expected rank corresponds to a higher expected performance. Controls: Health Status, Monthly HH income and Objective OI-Index.

Table 5.9: Overborrowing and Overspending

	Overconfidence	Overborrowing		Overspending
	(1)	(2)	(3)	(4)
Treatment	1.217***	0.010	−0.007	−0.034
	(0.284)	(0.019)	(0.019)	(0.045)
Overconfidence			0.014***	0.044***
			(0.004)	(0.007)
Controls	Yes	Yes	Yes	Yes
Observations	470	470	470	470

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Robust standard errors in parentheses. Treatment: 0=Hard Quiz, 1=Easy Quiz; Controls: Health Status, Monthly HH income and Objective OI-Index.

Summarized, our treatments shifted expectations in hypothesized directions; expectations are positively related to spending behavior, but the treatment has no impact on the latter. Therefore, we cannot claim that there is a causal link between expectations and overborrowing in our experiment.

5.4.3 Confounding Factors

The previous findings are robust to various restrictions. For example, they are not driven by participants who are very old or have mild comprehension difficulties

(we already excluded those with large difficulties in the main analysis). It is also not the case that the treatments only affect expected ranks but not expected earnings.[35] This suggests that there are confounding factors or "noise" interfering with our treatments. We run further analyses to rule out that the treatments affected factors other than expectations:

Frustration and Gratification. One of the most likely confounds could be that participants in the hard treatment feel frustrated because of the difficult questions and want to treat themselves with "shopping." In contrast, some others might be proud of mastering such a hard quiz and also want to reward themselves. Both motives should lead to the result that, specifically, participants with extreme expectations behave differently across treatments. Participants who are frustrated should rank themselves rather low whereas participants that are proud should rank themselves rather high. Subsequently, the buying behavior of participants with the same expected rank across treatments should be significantly different for the lowest and highest ranks. However, the only (marginally) significant difference we can detect is for the five participants who expected to reach rank two: here, participants in the hard treatment want to buy more than participants in the easy treatment. Excluding these observations does not change our results. For all other ranks, participants in both treatments exhibit the same spending pattern. This finding does not favor frustration and gratification as being possible confounding factors.

Temptation. Another possibility is that participants in the hard treatment are more susceptible to temptation goods. They have to exercise more cognitive effort, which decreases their self-control, so-called "ego depletion" (see, for example, Hagger et al., 2010). Running separate regressions on each product, we find a significantly different treatment effect only for dried mango. Still, self-control (measured with the scale from Tangney et al., 2004) and BMI do not have significant effects on buying mango, which opposes the ego depletion interpretation. We also do not find evidence that frustrated (more depleted) participants are more likely to buy mango. Furthermore, detergent is the most popular product and the share of detergent in all goods desired is not different across treatments, whereas mango is the least popular. Detergent is the one product we would expect to be least related to self-control issues. Summarized, we do not find con-

[35] This could happen if there is a piecewise treatment effect (shifting expectations only within the same earnings category) because earnings are only piecewise increasing in ranks and not equidistant.

vincing evidence that persons in the hard treatment are more likely to give in to temptation.

Based on these tests, we argue that we can rule out the most probable factors interfering with our treatment. We believe that the reason we do not find a treatment effect on spending and borrowing is that the shift in beliefs was not strong enough to eventually be reflected in spending. We find additional evidence for this proposition when employing IV estimation, where we instrument expected rank with treatment. Several tests indicate that treatment is a weak instrument for expected rank.

5.4.4 Behavior in the Lab and in Real Life

A supplementary result we find worth mentioning is that being over-indebted in "real life" is actually related to spending behavior in our experiment (see Table 5.10). Those respondents who have problems controlling their spending in real life are also those who spend less carefully in the game. Eventually, we see this as evidence that our experiment, although highly artificial, still captures aspects of real life behavior.

Table 5.10: Overborrowing in the Game and in Real Life

	No. Goods		Overborrowing		Overspending	
	(1)	(2)	(3)	(4)	(5)	(6)
Obj. OI-Index	−0.000		−0.001		0.050**	
	(0.077)		(0.008)		(0.021)	
Subj. OI-Index		0.105		−0.005		0.043*
		(0.078)		(0.008)		(0.022)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471	471	471	471	471	471

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Robust standard errors in parentheses. Controlled for confidence as defined in Equation 5.3.

We can only speculate why the well-established hard-easy gap is so small in our setting. Consulting our interviewers, we have no reason to believe that participants did not perceive the test quizzes as hard or easy when they should. Several other studies find larger shifts in beliefs, although participants had less

exposure to manipulation.[36] The rural Thai population may have more persistent beliefs than WEIRD populations. This makes changing these beliefs more difficult. Given the tremendous level of overconfidence we find in the lab, this circumstance might not be beneficial for our participants. It relates to our regression result that being too certain about future income is related to over-indebtedness. "Sticky," biased expectations, bear implications for policy making and must be taken into account when measures to reduce over-indebtedness are designed.

5.5 Conclusion

Over-indebtedness can pose a serious threat to households' welfare and the financial stability of a country, especially in emerging markets. However, the determinants underlying over-indebtedness globally are, so far, not well understood. Theoretically, as modelled in various permanent income hypotheses, higher income expectations should lead to a higher level of borrowing.

In this study, we analyze the relationship between high income expectations and over-indebtedness using data from an extensive household survey and a lab-in-the-field experiment. Low levels of financial knowledge and high income uncertainty demand for explicit research in emerging countries because relying on results for Western populations is insufficient. Our sample belongs to a panel survey of relatively poor and rural households in Thailand. Indeed, we can confirm a low level of financial literacy in several dimensions and find substantial uncertainty in income expectations for our sample. While over-indebtedness is increasingly recognized as a growing problem in Thailand, our study sheds light on one potential driver.

In our regression analysis, we find a strong and robust positive relationship between high expectations concerning future income and over-indebtedness controlling for various household characteristics and shocks. We think this is a sign that these expectations are actually too high for some households. This finding holds for various measures of over-indebtedness. They are stronger for objective measures, if we use a quantitative elicitation method for positive income expectations based on probabilistic expectations and stronger for subjective over-indebtedness, if we use a qualitative, more subjective forecast error. In any case, they are always significant. The results reflect that subjective over-indebtedness indicators are likely to be influenced more heavily by personal perceptions on

[36] For example, Grohmann et al. (2019) only use four questions they frame as "example questions" and find larger treatment effects on expectations.

the household's financial situation as well as by respondents' personality traits and that objective and subjective measures capture different dimensions of over-indebtedness. Eventually, higher certainty about the future household income development is also related to more household over-indebtedness, which might be the case because being too certain is not optimal given the highly uncertain environment. The results are robust to a diverse set of different sample specifications and we do not find evidence of reverse causality issues.

We attempt to establish a causal relationship between overconfidence as a form of biased expectation and overborrowing in our experiment by exogenously biasing self-confidence via the so-called hard-easy gap. Thereby, we change expectations about the future payout in the game. Our results show that, in the experiment, overconfidence is related to more spending and overborrowing, but we cannot claim causality. The most probable reason why our treatments do not affect spending behavior are too "sticky" beliefs. This also suggests that rural households are indeed too certain about their income expectations. Interestingly, we find that overspending in the experiment is related to overspending in real life, which confirms that the artificial experiment still captures real life behavior.

As we will never know the true income generating process, we cannot know whether the expectations of our respondents are systematically biased or positive for other reasons. A systematic overestimation of future income would have much more devastating effects than a random, one-shot, inaccurate guess. Nevertheless, we find reassuring evidence that even one-time high expectations are positively related to household over-indebtedness, thus pushing households into severe poverty. One of the potential channels through which high expectations are related to over-indebtedness is being too certain about own expectations in the highly uncertain environment that rural households in emerging markets are living in. Given the supplemental evidence for sticky beliefs from our experiment, to change beliefs or their certainty seems to be challenging. More appropriate policy measures might reduce vulnerability and uncertainty with the expansion of assistance and insurance schemes, especially for households engaged in agriculture.

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Appendix A to accompany Chapter 2

A.1 Additional Figures

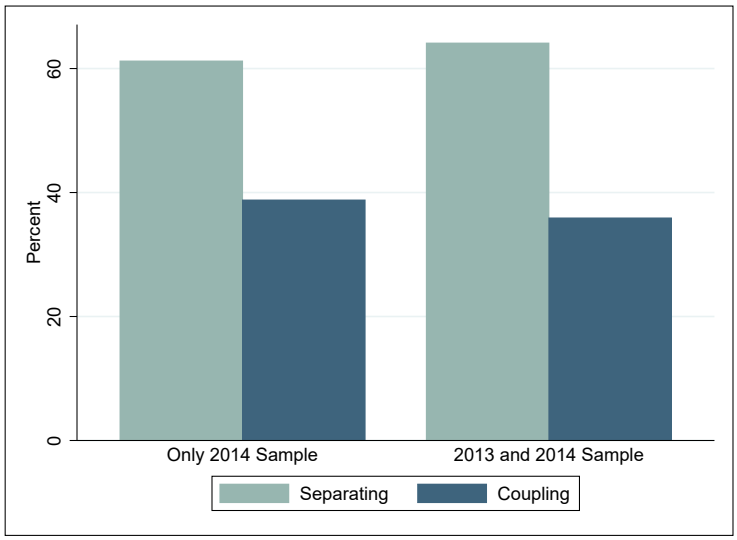


Figure A.1.1: Share of Respondents Coupling vs Separating by Survey Participation

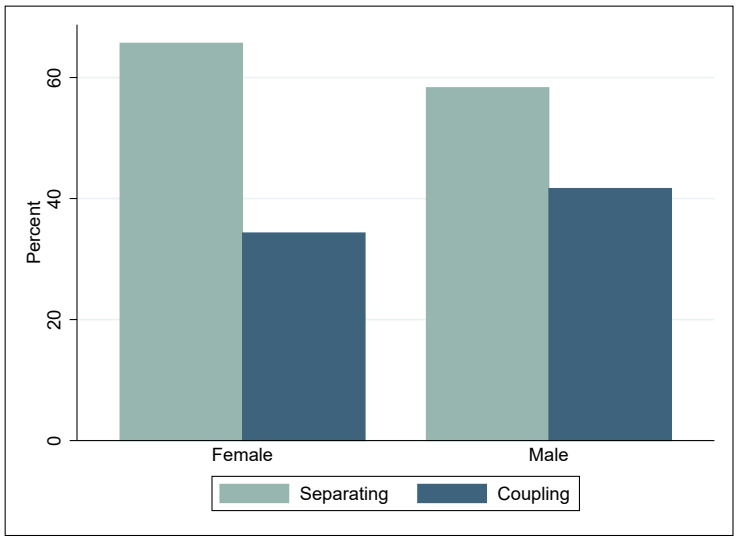


Figure A.1.2: Share of Respondents Coupling vs Separating by Sex

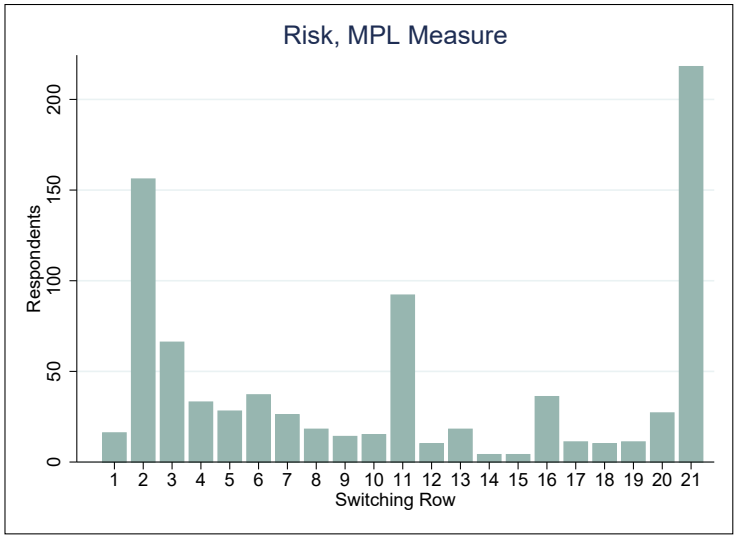


Figure A.1.3: Switching Row, from Lottery to Safe Amount in Multiple Price List Game

A.2 Material

Material A.2.1: Introductory Statement for the Games Section of the Survey

SECTION 7 – Risk Games

HOW ARE YOU FEELING NOW?
Very good_____01
Good_____02
Quite ok_____03
Not so good_____04
Bad_____05

General Instructions *(Enumerator: read out)*

Now we would like to invite you to play 4 games. The first game is about choosing a lottery or a safe amount; the second game is about choosing a sooner or a later amount, the third game is about rolling a dice and to memorize the number of eyes; the fourth one is about sharing the benefits together with a neighbor. After you are finished with playing the 4 games, you will play one game for real money. Therefore you are asked to blindly draw one out of four numbers from 1 to 4) to determine which of these 4 games is paid out in real money.

For each of these 4 games we will show you cards that contain all information for you to make decision. (*Enumerator: show participants the 4 show cards*)
We will note your choice for each question in our record sheets.

1 Do you agree to play these games with us?

Yes_____01 (go to section 7.1)
No_____02 (fill in next question and proceed with section 8)

2 In case you decide not to participate in the risk games please tell us why you do not want to participate?

Religion_____01
Bad experience with risk games_____02
Never play games_____03
Others, please specify_____04

Material A.2.2: Instructions for Coupled Lotteries in the Survey

There is a 50:50 chance to win 100 THB. Do you want to win or lose alone or together? (Enumerator, please tell the respondent that this game is played together with respondent's neighbor. The 50:50 chance to win 100 THB will be determined by a flip of a coin. Ask both the respondent and his neighbor if they like to take the chance to win or lose together or if they like to take the chance to win or lose alone. Pass them the attached extra sheets and let them secretly circle their answer.)

1 Hidden choice (Enumerator, please tick the box according to the hidden choice of the respondent and of the neighbor)

	Like to play alone	Like to play together
Respondent		
Neighbor		

Material A.2.3: Decision Sheets for Respondent and Neighbor

Hidden choice: Respondent

Please tick how do you like to play the following game:

The coin decides if you win 100THB ("King") or 0 THB ("Palace")

Do you like to play **alone**? ☐

Or

Do you like to play **together** with your neighbour? ☐

Hidden choice: Neighbour

Please tick how do you like to play the following game:

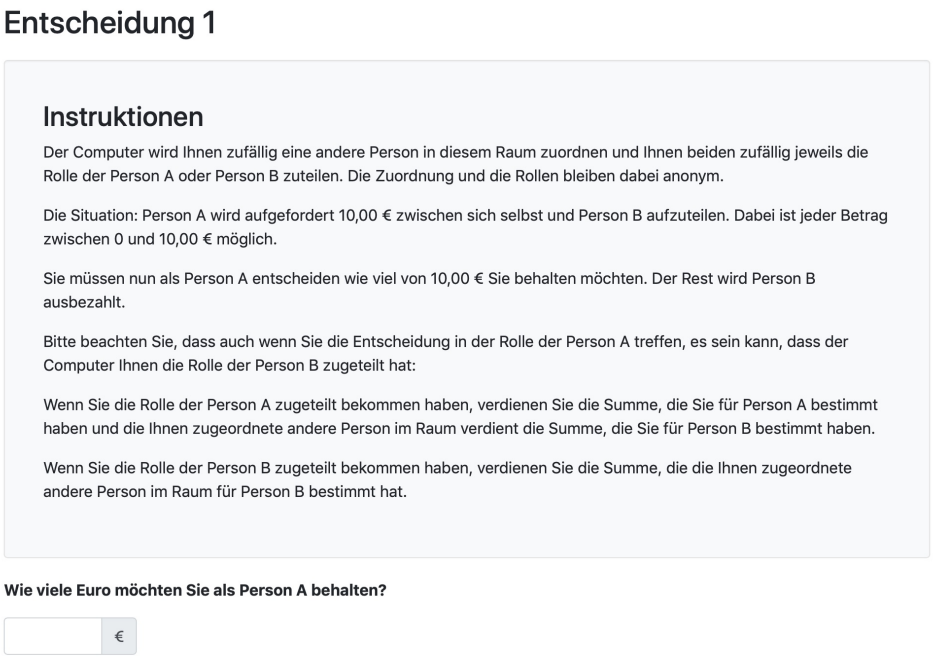
The coin decides if you win 100THB ("King") or 0 THB ("Palace")

Do you like to play **alone**? ☐

Or

Do you like to play **together** with your neighbour? ☐

Material A.2.4: Instructions for Dictator Game in the Lab



[Translation:] **Decision 1**
The computer will match you to a random person in this room and randomly assign you to the roles of person A or B. The matching and assignment of roles will be kept anonymous.

The setup: Person A will be asked to distribute 10€ between herself/himself and person B. Each amount between 0 and 10€ is possible.

You have to decide now as person A how much of these 10€ you want to keep. The remaining amount is paid to person B.

Please note that even though you make the decision as person A now, it might happen that the computer assigns you the role of person B:

If you were assigned the role of person A, you will receive the amount that you assigned to person A and the other person in the room who is matched to you receives the amount you assigned to person B.

If you were assigned the role of person B, you will receive the amount that the other person in the room who is matched to you assigned to person B.

How much do you want to keep as person A?

Material A.2.5: Instructions for Coupled Lotteries in the Lab

[Translation:] **Coupling of Payoffs**
Again, the computer will match you to a random person in this room and randomly assign you to the roles of person A or B. Please note that this matching is a completely new (and random) matching. Matching and assignment are again anonymous.

The setup: A virtual coin flip will decide whether you will gain 10€ or nothing. This means, you have a 50:50 chance to win 10€. Person B is in the same situation. Both of you have to decide now whether you take the chance to win together or alone. "Together" means a single coin flip will decide for both of you if EACH of you will win 10€ or nothing respectively. "Alone" means that two independent coin flips will decide for each of you separately whether you will win 10€ or nothing.

Please note that the roles of person A and B are exactly the same here.

If you decide unanimously, this decision will be implemented.

If you decide differently, there will be a third coin flip deciding whether it will be played alone or together.

Do you want to play alone or together with person B?

Material A.2.6: Multiple Price List for Gains - Lab Experiment

Lotterie

Bitte entscheiden Sie nun zwischen zwei Optionen. Entweder bestimmt ein Münzwurf, ob Sie €0 oder 10,00 € erhalten. Oder Sie wählen eine sichere Zahlung mit ansteigenden Beträgen.

	Lotterie			Sichere Zahlung
1	50%: 10,00 €; 50%: 0€	◉	○	€0.5
2	50%: 10,00 €; 50%: 0€	◉	○	€1
3	50%: 10,00 €; 50%: 0€	◉	○	€1.5
4	50%: 10,00 €; 50%: 0€	◉	○	€2
5	50%: 10,00 €; 50%: 0€	◉	○	€2.5
6	50%: 10,00 €; 50%: 0€	◉	○	€3
7	50%: 10,00 €; 50%: 0€	◉	○	€3.5
8	50%: 10,00 €; 50%: 0€	◉	○	€4
9	50%: 10,00 €; 50%: 0€	◉	○	€4.5
10	50%: 10,00 €; 50%: 0€	○	◉	€5
11	50%: 10,00 €; 50%: 0€	○	◉	€5.5
12	50%: 10,00 €; 50%: 0€	○	◉	€6
13	50%: 10,00 €; 50%: 0€	○	◉	€6.5
14	50%: 10,00 €; 50%: 0€	○	◉	€7
15	50%: 10,00 €; 50%: 0€	○	◉	€7.5
16	50%: 10,00 €; 50%: 0€	○	◉	€8
17	50%: 10,00 €; 50%: 0€	○	◉	€8.5
18	50%: 10,00 €; 50%: 0€	○	◉	€9
19	50%: 10,00 €; 50%: 0€	○	◉	€9.5

Material A.2.7: Questionnaire - Lab Experiment

What is your sex?

- Male
- Female

What is your age?

___ years

What is your body-height?

___ cm

What is your body-weight?

___ kg

Are you a person who rather avoids risks or are you rather willing to take risks?

- Mostly avoid risks
- Rather avoid risks
- Rather willing to take risks
- Mostly willing to take risks

Do you frequently take advice from other students, if you have to make decisions regarding your studies?

- Yes
- No

How many persons are so close to you that you can count on them if you are in serious trouble (e.g. illness, lovesickness, stress)?

- No one
- 1 or 2
- 3 - 5
- 6 or more

Generally speaking, would you say that most people can be trusted or that you can't be too careful when dealing with people?

- Most people can be trusted
- You can't be too careful when dealing with people

Do you frequently compare your qualities or performance with other persons?

- Yes
- No

How happy are you with your life in general?

- Very unhappy
- Rather unhappy
- Rather happy
- Very happy

How much money do you have available each month in comparison to other students?

- Much less
- A little bit less
- About the same
- A little bit more
- Much more

And now a question about your donations. We understand donations here as giving money for social, church, cultural, community, and charitable aims, without receiving any direct compensation in return. These donations can be large sums of money but also smaller sums, for example, the change one puts into a collection box. We also count church offerings. Did you donate money last year, in 2017 – not counting membership fees?

- Yes
- No

If yes: How high was the total sum of money that you donated last year?

___ Euro

Suppose you put 100€ into your savings account. The interest rate is 2% per year and you leave this money on your account for 5 years. What do you think: how much money would be in the account after 5 years?

- More than 102€
- Exactly 102€
- Less than 102€
- Don’t know

Suppose the interest rate on your savings account is 1% per year, and the inflation rate per year is 2%. What do you think: after 1 year, can you buy the same, more or less than today?

- More
- Exactly the same
- Less
- Don’t know

Is the following statement true or false?
“Buying a company stock usually provides a safer return than a stock mutual fund. “

- True
- False
- Don’t know

A.3 Description of Independent Variables

Male	Sex of respondent, 0=Female, 1=Male
Age	Age of respondent in years
Years of Schooling	Years respondent went to school
Ann. Consumption per Cap. (log)	Log annual consumption per capita in THB
Ann. Income per Capita	Annual household income in 2013 USD
Assets Value	Total value of all durable goods in the household in THB
Risk Taking, MPL	Risk preference measured via Multiple price list method, variable indicates the switching row from the lottery to the safe amount, rows from "1"-"21(Never)". The lottery gives 300 THB with 50% chance and 0 THB with 50% chance, the safe amount increases gradually from 0 to 190 THB.
General Risk Taking	Answer to "Are you generally a person who is willing to take risks or do you try to avoid taking risk?", from 0-"Fully unwilling to take risks" to 10-"Fully willing to take risks"
Hypothetical Investment	Amount of money respondents would invest if they would win 100,000 THB and the chance is 50% that the investment is doubled and 50% that it is halved
Comparing with Neighbors	Dummy for neighbors being the main reference group respondents compare their standard of living with
Not Comparing	Dummy for not comparing oneself's standard of living to other persons
Advice from Neighbor	Dummy whether respondents take agricultural advice from neighbors/relatives
Trust in Village	Believe in trustworthiness of other persons in the own village from 1-"Trust them not at all"to 4-"Trust them a lot"

Numeracy	Counts the number of right answers to following questions:
1	What is 45 + 72?
2	You have 4 friends and you want to give each friend 4 sweets. How many sweets do you need?
3	What is 5% of 200?
4	You want to buy a bag of rice that costs 270 Baht, but you only have one 1000 Baht note. How much change will you get back?
5	In a sale, a shop is selling all items at half price. Before the sale, a mattress costs 3000 Baht. How much will the mattress cost in the sale?
6	A second-hand motorbike dealer is selling a motorbike for 12000 Baht. His is two thirds of what a new motorbike costs new. How much did the new motorbike cost?
Financial Literacy	Counts the number of right answers to following questions:
Interest Rates	If today you borrow 10,000 THB, at an interest rate of 2% per month, after 3 months how much do you owe totally (principle + interest)? Less than 10,200 THB More than 10,200 THB Exactly 10,200 THB
Inflation	If you have 10,000 THB in an account, the interest rate on the account is 10% per year, and during this time, the price of goods and services rises by 12% per year, after one year you can buy: Less than you can buy today More than you can buy today Exactly the same as today
Expected Utility	For the same amount of money, a person can enter either one these two lotteries. Lottery A pays a prize of 2,000 THB, and the chance of winning is 5%. Lottery B pays a prize of 100 THB, and the chance of winning is 10%. Which Lottery pays the higher expected amount? Lottery A Lottery B Two lotteries pay the same expected amount
Loan Conditions	Suppose you need to borrow 50,000 THB. Two people offer you two different loans, the first loan you have to pay back 60,000 THB in one month, with the second loan you have to pay back 50,000 THB plus 15% in one month. Which loan is the better option?

Household Nucleus Size	Number of persons living in the household for most time of the year
Self-Employed	Dummy whether the household head is self-employed including being engaged in agriculture
HH better off in 5 years	Answer to "Do you think your household will be better off in 5 years?", from 1-"Much better off" to 5-"Much worse off"
Number Anticipated Risks	Number of household risks a household head thinks will occur in the next 5 years from a predefined list of 17 different risk plus own entries possible
Number Weather Risks	Number of household risks a household head thinks will occur in the next 5 years that are related to weather
Land Size	Size of the area used for cultivating crops in Rai (1600 sq m)
Number of Tractors	Number of tractors in the household
Number of Waterpumps	Number of waterpumps in the household
Advice from Classmate	Dummy whether participants take study advice from classmates
Dictator Keeping	Amount of that participant wants to keep in the dictator game. Can take values between 0 and 10(€).
Relative Income	How much disposable money per month participants think they have in comparison to other students on a scale from 1-"Much less" to 5-"Much more"
Compare Performance	Dummy whether participants often compare their attributes and performance with other persons
Happiness	How happy participants are in their lives from 1-"Very unhappy" to 4-"Very happy"

Appendix B to accompany Chapter 3

B.1 Susceptibility to Social Comparison

In the following subsections, we present all the hypotheses as stated in our pre-analysis plan, including those on which personality characteristics are more or less susceptible to social comparison effects. Subsequently, we discuss the results on the personality types.

B.1.1 Hypotheses

To answer our main research question, we look at the difference in the amount and the probability of loans taken between those in the private treatment and those in the other two treatments. Thus, these variables focus on the differences between the amount that someone should have spent according to standard economic predictions and the amount that someone actually spends. Furthermore, we look at within subject differences in what participants reported to be their quality preference for the pen in our online survey and what they actually buy during the experiment. To assess which personalities are more susceptible to social comparison effects, we interact the personality traits with our treatments. Finally, we also analyze the amount of effort exerted in the slider task to investigate who is willing to work more in the future to actually avoid financial distress because of socially contingent consumption.

Question 1: *"Are people willing to pay out of their future income because of social image/status concerns?"*

We expect that at least some people are willing to do so. As previously explained, buying a low quality pen is a credible signal for being a "lower" cognitive ability type, as it can be directly linked to worse performance in the test of intelligence. Since we assume that cognitive ability is a desirable trait for our student sample, for some persons the additional benefit of being perceived as having higher cognitive ability is large enough to offset the potential costs of borrowing or of "working more" (see hypothesis 1a). In our experiment, participants can borrow money without interest, reducing potential costs of borrowing to general opportunity costs of spending more instead of keeping money. Some participants in the public treatment are, thus, willing to use their future income to buy a higher quality pen than they can afford in order to hide their true performance. Since social image concerns can only arise when individual decisions are made publicly, these concerns neither arise in the private nor the information treatment.

Hypothesis 1: *"Participants in the public treatment are more likely to take out a loan and take out a higher loan amount to buy a higher quality pen than participants in the private treatment."*

Participants in the public treatment who take out a loan, because they want to convey a certain type, end up with less money after the shopping round than their control treatment counterparts who cannot engage in socially contingent consumption. Assuming only weak fatigue, the marginal rate of substitution of not exerting effort in the slider task for money should be larger for those subjects, as they have a debt on their accounts. Differently speaking, persons who take out a loan might be willing to work more because they want to settle their

debts.[1] Determining if individuals with a loan exert more effort is interesting because, in real life, higher consumption could be financed by debt or by working more (e.g. Neumark and Postlewaite, 1998; Bowles and Park, 2005). Hence, some of our participants could already have internalized working more in the slider task to take out a higher loan.

Hypothesis 1a: *"Participants in the public treatment will exert more effort in the slider task than participants in the private treatment, because they took out a higher loan before."*

Question 2: *"Can the peer effect on visible consumption mostly linked to social image/status concerns partly be explained by peer information?"*

A different explanation why people adjust their consumption to peers is that they are intrinsically motivated or because they receive information about the usefulness/quality of a product. Intrinsic motivation could be a form of self-image concern, a desire to imitate or a desire to conform to others. Pure information about the quality is especially important if the individual is not familiar with the product. To analyze whether the effect of peer information is comparable to that of social image concerns, we designed the information treatment in such a way that only new information but no social image concerns can arise.[2] Our prediction is that peer information only has a small effect on the decisions in our setting. The pens we use are trivial goods and quality differences are comparatively small, which is why we expect the intrinsic and informational gain to be small. However, we acknowledge that this is not necessarily true for goods that are usually considered in field studies on conspicuous consumption, e.g. cars, travel destinations, restaurant visits, and so forth. In this sense, our treatment for information effects lies at the lower bound. Finding significant results would possibly imply that a substantial share of visible consumption is actually not driven by conspicuous consumption.

Hypothesis 2: *"Participants in the information treatment will take out a higher loan than participants in the private treatment, but a smaller loan than participants in the public treatment."*

Hypothesis 2a: *"Participants in the information treatment will exert more effort in the slider task than participants in the private treatment, but less than participants in the public treatment."*

Question 3: *"Are there certain types of personality that correlate with larger socially contingent consumption?"*

Since cognitive reflection is related to standard IQ measures, we expect small effects in our setting. Participants with high cognitive reflection are expected to perform well in our intelligence task and, therefore, can buy high quality pens without needing to take out a loan. This reduces

[1] An alternative explanation would be that these persons do not want to lose money they already have in their mental accounts. They do not like the feeling of creating a debt that eventually will be deducted from their participation fee, which is already part of their endowment.

[2] Given our experimental design, observations in the information treatment within a session are path dependent. We try to control for this issue in our analysis.

the difference between the control and the other treatments. Nevertheless, we hypothesize to find a negative relation between CR and susceptibility to social image concerns after controlling for performance. Royzman et al. (2014) find that moral values of reflective persons are more independent of existing social norms. We see this as indication of putting less value on what other people think about oneself.

Hypothesis 3a: *"Participants with higher cognitive reflection are less susceptible to social image concerns."*

We expect higher internal locus of control to decrease the reliance on social networks and perceived peer pressure, because it relates to the belief that individuals are responsible for their lives themselves.

Hypothesis 3b: *"Participants with rather internal locus of control are less susceptible to social image concerns."*

Self-esteem and power, the capability to control other people, are related concepts and power affects self-esteem (Wojciszke and Struzynska-Kujalowicz, 2007). Since research shows that feeling powerful decreases conspicuous consumption, we expect an analogous effect for self-esteem.

Hypothesis 3c: *"Participants with higher self-esteem are less susceptible to social image concerns."*

High self-monitors adjust their self-presentation more than low self-monitors to signal a desired type. High self-monitors have a more precise estimate of the social desirability of an action and care more about being perceived as a higher type.

Hypothesis 3d: *"Participants with higher self-monitoring are more susceptible to social image concerns."*

Looking at the Big Five personality traits, we concentrate on the traits of extraversion, openness, and agreeableness. For the remaining two traits, we do not have a clear prediction. Extraversion is shown to be positively correlated to status consumption of low status individuals (Landis and Gladstone, 2017). Therefore, we expect it to be related to social image concerns. For openness and agreeableness, we only formulate hypotheses regarding their effect on responding to peer information. A high level of openness means to be open to new experiences, ideas, and variety seeking. Therefore, openness drives participants away from the mean decision of others, which is considered as not innovative and unexciting. Agreeableness is closely related to the desire for conformity and cooperation, which is why we predict it to be related to anchoring the own decision on others' decisions.

Hypothesis 3e: *"Participants with a higher level of extraversion are more susceptible to social image concerns."*

Hypothesis 3f: *"Participants with a higher level of openness will anchor their decision less to the average decision in the information treatment than those with a lower level."*

Hypothesis 3g: *"Participants with a higher level of agreeableness will anchor their decision closer to the average decision in the information treatment than those with a lower level."*

B.1.2 Results on Personality Types

For all characteristics listed in Table B.1.1, we only derived hypotheses for the interaction with the public treatment and, therefore, do not report coefficients for the information treatment. We first look at cognitive reflection (CR). Since we find a highly significant correlation between CR and actual performance in the IQ-quiz, we additionally control for performance. As expected, a better performance is significantly negatively correlated with taking a loan. Interestingly, for the control treatment, a higher CR is significantly positively related to loan take-up. However, we find a negative effect of cognitive reflection on loan take-up in the public treatment. The effect is rather small and only marginally significant on the extensive margin. However, if we do not control for possibly endogenous self-esteem, the effects are stronger and highly significant. In general, the interaction effect is robust to various specifications and more than offsets the positive effect of CR in the control. Because this study is slightly under-powered to estimate effects of this size, we are still cautious in interpreting the results. Still, it seems that individuals with higher cognitive reflection do adjust their decision because of social image concerns, but in opposite direction to the others. In this sense, we have to reject hypothesis 3a.

The results for the interaction between public treatment and locus of control (LOC) are shown in the second panel of Table B.1.1. Internal LOC is also correlated with performance but to a smaller extent. We do not find a significant interaction effect for the probability to take up a loan at all, although the coefficient points in the right direction.[3] Given the rather large standard errors and the imbalance of LOC between control and public, we view our results as inconclusive. Thus, we also cannot confirm hypothesis 3b. For global self-esteem (GSE), we find an insignificant interaction term and a rather small effect size. As participants with higher GSE are overly represented in the public treatment or higher GSE might be induced by the treatment, we would expect larger effects in negative direction: The treatment could give those persons who performed well in the quiz a confidence boost, who can now announce this publicly (and vice versa). Actually, there is a mild correlation between quiz performance and GSE. However, this should increase the effect size in favor of our hypothesis, which is not the case. Thus, we reject hypothesis 3c.

Similar as for GSE, we do not find any effect for self-monitoring (SM). The effect size is fairly small and effects are not significant at all. Interestingly, SM is negatively correlated to quiz performance, which even should increase the potential effect. Based on these results, we also reject hypothesis 3d. The last panel in the table presents the results on extraversion (EV). The effects go in hypothesized direction, but are never significant. Effect sizes, though, are of moderate size (Cohen's d $\sim$ 0.24) and p-values are "flirting with significance." As previously noted, our study is under-powered for this effect size and, hence, we are hesitant to reject hypothesis 3e but also cannot confirm it, which means results are inconclusive.

[3] If we apply wild cluster bootstrap, we additionally find an overall significant positive effect of LOC at the 10% level.

Table B.1.1: Personality and Loan Take-Up

	Loan Amount	Loan Dummy
Cognitive Reflection		
CR	0.100***	0.094***
	(0.031)	(0.028)
Interaction CR*Public	−0.128	−0.116*
	(0.084)	(0.065)
Public Treatment	0.249	0.244
	(0.210)	(0.153)
Locus of Control		
LOC	0.056	0.054
	(0.082)	(0.036)
Interaction LOC*Public	−0.094	−0.101
	(0.104)	(0.064)
Public Treatment	−0.012	0.013
	(0.138)	(0.093)
Self-Esteem		
GSE	−0.100	−0.049
	(0.080)	(0.071)
Interaction GSE*Public	−0.047	−0.027
	(0.131)	(0.092)
Public Treatment	0.006	0.023
	(0.146)	(0.102)
Self-Monitoring		
SM	0.046	0.023
	(0.049)	(0.036)
Interaction SM*Public	0.017	0.020
	(0.072)	(0.069)
Public Treatment	−0.016	0.003
	(0.137)	(0.096)
Extraversion		
EV	−0.030	−0.001
	(0.055)	(0.048)
Interaction EV*Public	0.148	0.093
	(0.098)	(0.066)
Public Treatment	−0.010	0.013
	(0.134)	(0.098)
Observations	248	248

Control treatment is reference category. Coefficients on info treatment not reported. Controlled for variables with significant differences and performance in IQ-quiz. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Hypotheses 3f and 3g address the anchoring of decisions to others in the information treatment. To measure anchoring, we again look at the difference between pre-experiment choice and actual choice, interacting the information treatment with the two personality traits. In Table B.1.2, we first investigate whether persons with a higher level of openness deviate less from their individual preference as they receive information about others.

Table B.1.2: Personality and Adjustment

	Pen Before	Pen After	Difference
Openness			
OP	0.288**	0.018	−0.316**
	(0.127)	(0.063)	(0.127)
Interaction OP*Info	−0.372*	0.168	0.580**
	(0.215)	(0.107)	(0.263)
Info Treatment	−0.187	−0.010	0.162
	(0.245)	(0.149)	(0.258)
Agreeableness			
AG	0.118	0.083	0.009
	(0.158)	(0.124)	(0.159)
Interaction AG*Info	−0.162	−0.154	−0.057
	(0.252)	(0.185)	(0.327)
Info Treatment	−0.235	0.017	0.255
	(0.257)	(0.195)	(0.300)
Observations	201	248	201

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences and performance in IQ-quiz. SE in parentheses, bootstrapped and clustered on session level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

First, we notice a strange differential effect for the pre-experimental choice. In general, a higher level of openness is related to choosing a more expensive pen in the online survey but the interaction has a large significantly negative effect. Eventually, persons with a higher level of openness who were assigned to the information treatment choose a cheaper pen in the survey. However, as treatment assignment is random, this is most likely an artifact of the small sample size. Nevertheless, we find a considerably large positive effect for the actual choice and the difference between the choice before and during the experiment. This means that we have to reject hypothesis 3f, as apparently it is exactly the opposite: persons with a higher level of openness deviate more from their pre-experiment preference.

Panel 2 in Table B.1.2, shows the effect of agreeableness on anchoring. Here, we do not find significant effects and the difference between pre-experimental and actual choice is small in size. Therefore, we also reject hypothesis 3g.

B.2 Additional Results

Figure B.2.1: Pre-Experimental Choices - Pens, Lip-Balms, and Folders

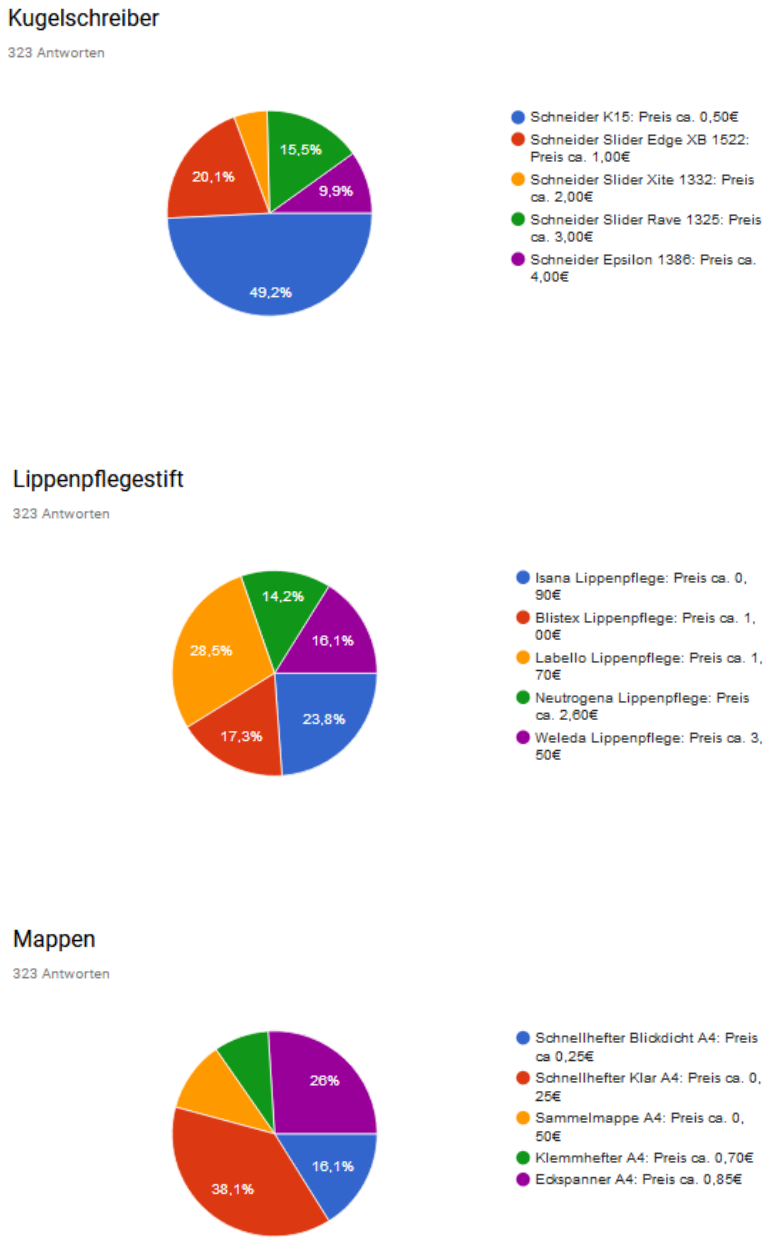


Table B.2.1: Descriptive Statistics across Survey Participation

	Full Sample	Online Survey	No Survey	Difference
Male	0.48	0.47	0.52	0.05
Age	22.86	22.87	22.80	−0.06
Education	3.36	3.36	3.35	−0.00
Students	0.97	0.96	0.98	0.02
Semester	3.68	3.51	4.39	0.88
Student Job	0.28	0.26	0.33	0.07
Mthl. Income	688.36	692.70	669.86	−22.84
Risk Preference	5.34	5.39	5.15	−0.24
Lab Experience	1.89	1.87	1.96	0.09
Know Someone	0.31	0.34	0.20	−0.14*
Persons in Session	11.33	11.26	11.61	0.35***
Correct Control Questions	4.74	4.76	4.67	−0.09
Correct Quiz Questions	4.43	4.43	4.43	0.00
Financial Literacy	4.59	4.60	4.53	−0.07
Cognitive Reflection	1.91	1.91	1.90	−0.01
Conscientiousness	−0.01	−0.01	−0.01	0.00
Neuroticism	0.03	−0.01	0.20	0.21
Extraversion	−0.02	−0.03	0.01	0.04
Openess	−0.02	−0.00	−0.11	−0.11
Agreeableness	−0.01	0.02	−0.16	−0.18
Self-Esteem	−0.01	0.03	−0.18	−0.21
Locus of Control	0.00	0.05	−0.21	−0.26
Self-Monitoring	−0.02	−0.04	0.09	0.14
Observations	270	219	51	270

* p < 0.10, ** p < 0.05, *** p < 0.01

Table B.2.2: Effects on Loan Take-Up - Pre-Experiment Choice

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Public Treatment	−0.068	−0.006	−0.083	−0.021
	(0.125)	(0.088)	(0.146)	(0.094)
Info Treatment	0.078	0.030	0.036	−0.017
	(0.135)	(0.076)	(0.133)	(0.079)
Pen Before	0.084**	0.056***	0.064*	0.039
	(0.036)	(0.021)	(0.038)	(0.025)
Mean Control Group	0.220	0.172	0.220	0.172
Controls	No	No	Yes	Yes
Observations	219	219	201	201

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

* p < 0.10, ** p < 0.05, *** p < 0.01

Table B.2.3: Effects on Loan Take-Up, Info Treatment Correction - Pre-Experiment Choice

	Loan Amount (1)	Loan Dummy (2)	Loan Amount (3)	Loan Dummy (4)
Info Treatment	−0.151	−0.165	0.031	0.180
	(0.249)	(0.109)	(0.586)	(0.476)
Order	−0.016	−0.002		
	(0.022)	(0.010)		
Interaction Order*Info	0.030	0.024*		
	(0.027)	(0.013)		
Pen Before	0.067*	0.040	0.014	0.019
	(0.038)	(0.026)	(0.032)	(0.027)
Mean Prev. Pens			0.086	0.124
			(0.137)	(0.141)
Interaction Mean*Info			−0.005	−0.067
			(0.211)	(0.175)
Mean Control Group	0.220	0.172	0.220	0.172
Correction	Order	Order	Mean Pen	Mean Pen
Observations	201	201	183	183

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2.4: Deviation from Pre-Experiment Choice - Dummy

	Pre-Experiment = Experiment			
	(1)	(2)	(3)	(4)
Public Treatment	−0.012	−0.051	0.198	0.216
	(0.066)	(0.075)	(0.165)	(0.195)
Info Treatment	−0.020	−0.027	0.103	0.135
	(0.080)	(0.100)	(0.170)	(0.201)
Order			0.012	0.015
			(0.015)	(0.014)
Interaction Order*Info			−0.020	−0.025
			(0.023)	(0.025)
Mean Control Group	0.312	0.312	0.312	0.312
Controls	No	Yes	No	Yes
Correction	No	No	Order	Order
Observations	219	201	219	201

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table B.2.5: Using a Different Ordering

	Loan Amount	Loan Dummy	Lost Amount	Lost Dummy
	(1)	(2)	(3)	(4)
Info Treatment	0.016	−0.161	0.094	0.138
	(0.193)	(0.113)	(0.085)	(0.103)
Order 2	0.004	−0.007	−0.004	−0.002
	(0.021)	(0.005)	(0.004)	(0.004)
Interaction Order*Info	0.007	0.025*	−0.003	−0.011
	(0.029)	(0.013)	(0.008)	(0.010)
Mean Control Group	0.220	0.172	0.038	0.043
Correction	Order 2	Order 2	Order 2	Order 2
Observations	248	248	248	248

Control treatment is reference category. Coefficients on public treatment not reported. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table B.2.6: Decisions by Sex

	Loan Amount (1)	Loan Dummy (2)	Lost Amount (3)	Lost Dummy (4)
Public Treatment	−0.009	0.034	0.097*	0.071
	(0.165)	(0.133)	(0.052)	(0.047)
Info Treatment	−0.009	−0.057	−0.002	0.010
	(0.161)	(0.121)	(0.047)	(0.051)
Male	0.066	−0.034	−0.060	−0.045
	(0.108)	(0.084)	(0.039)	(0.044)
Interaction Sex*Public	−0.035	−0.055	0.039	0.010
	(0.190)	(0.141)	(0.111)	(0.078)
Mean Control Group	0.220	0.172	0.038	0.043
Observations	247	247	247	247

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table B.2.7: Number of Modes for Pens Bought

	Number of Modes	Number of Modes
Public Treatment	−0.207	−0.191
	(0.573)	(0.580)
Info Treatment	−0.415	−0.641
	(0.431)	(0.451)
Mean Control Group	1.903	1.903
Controls	No	Yes
Observations	270	248

Control treatment is reference category. Controlled for variables with significant differences. SE in parentheses, bootstrapped and clustered on session level.
* p < 0.10, ** p < 0.05, *** p < 0.01

B.3 Experimental Material

Material B.3.1: Instructions

Instructions

The experiment in which you are going to participate serves to analyze decision behavior.

For your presence, you will receive an amount of 5 Euro, independent of your decisions and of other events in the experiment. The participation fee is 3.50 Euro. In addition, you can earn money in the experiment that depends on your decisions and on the decisions of the other participants. For that reason, it is very important that you read these instructions thoroughly.

During the experiment it is not permitted to use electronic devices or to communicate with the other participants as long as you are not requested to do so. Please only use the programs and functions provided for this experiment. Please do not talk to the other participants. If you have a question, please raise your hand. We will come to answer your question in private. Please do not ask your question out loud in any circumstance. In case the question is relevant for all participants, we will repeat it and answer it for everyone. If you violate the rules, you will be excluded from the experiment and the payment.

At the beginning of the experiment, you will find short comprehension questions on the screen. Please answer these. If you answer one or more of these questions incorrectly, one of the experimenters will come to discuss open questions with you if necessary.

Structure of the experiment:

1. First option to obtain income - IQ test questions
2. Information about your performance in the first revenue round
3. Shopping round
4. Second option to obtain income – Slider
5. Questionnaire

What happens during the first revenue round?

You have to answer 12 questions during the first revenue round. These are questions that are also used to measure intelligence. The income in this round depends on your performance in relation to the other participants. The three participants with the best results get 3 Euro, the second three get 2 Euro, the third three get 1 Euro and the last three get 0.50 Euro. This means, you are in a direct comparison with the other participants. In case of a tie, the speed with which the questions were answered decides over the ranking.

You will learn see how you performed in comparison to the other participants directly after the IQ test questions. You alone will see your personal rank.

What happens during the shopping round?

After the IQ test questions, you will have the possibility to buy a pen. You can decide between five different pens. All pens are of different quality and have different prices. If your earned income is not sufficient, you will have the opportunity to take out a loan to buy a pen of better

quality. All pens are clearly labelled and the quality of the pens is obvious. Income not spend will expire. The taken credit will be subtracted from your participation fee of 3.50 Euro. You will receive the pen at the end of the experiment together with your payment.

You will later see on the screen how you inform the experimenters about your decision.

What happens during the second revenue round?

In this round, you can earn additional income. Your income will depend solely on your own performance. You have to move sliders to a certain point. You will be paid for each slider that is moved to the right point. The income you will earn per slider will decrease with the amount of sliders you already set correctly: for the first set of eight correctly set sliders you earn more than for the second set of eight correctly set sliders, for the second set of eight correctly set sliders you earn more than for the third set of eight correctly set sliders etc.. You can keep the whole income you earned during this round.

This round follows a questionnaire. At the end of the experiment, you will receive your payment and the pen you bought in the next room one after another. Please wait outside the room until we call your name as only one person at the same time should be inside the room to receive the payment.

Schematic:

Total remuneration = Show-Up fee 5 Euro

+ Participation fee 3.50 Euro

+ Variable income 1 (IQ test: must be spent to purchase a pen or expires)

+ Variable income 2 (Slider task: money can be kept)

Material B.3.2: Comprehension Questions

Comprehension questions:

1. On what does your **income depend** in the **first revenue round**?
    a. Only on my own performance
    b. On my own performance in relation to other participants
    c. Only on the performance of the others

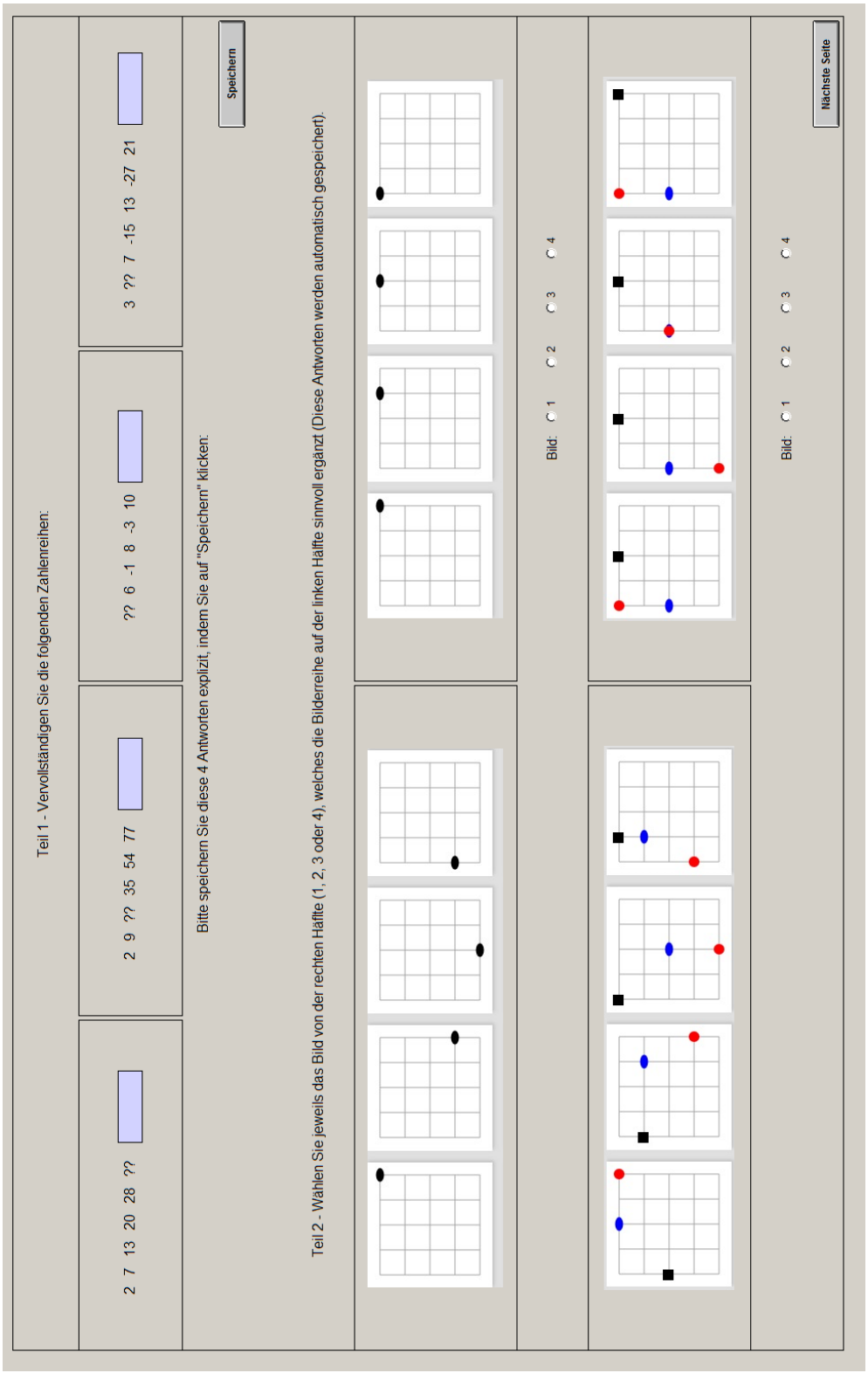
2. What happens to the **income** of the first round that **you do not spend**?
    a. I can keep it
    b. It expires
    c. The other participants get it

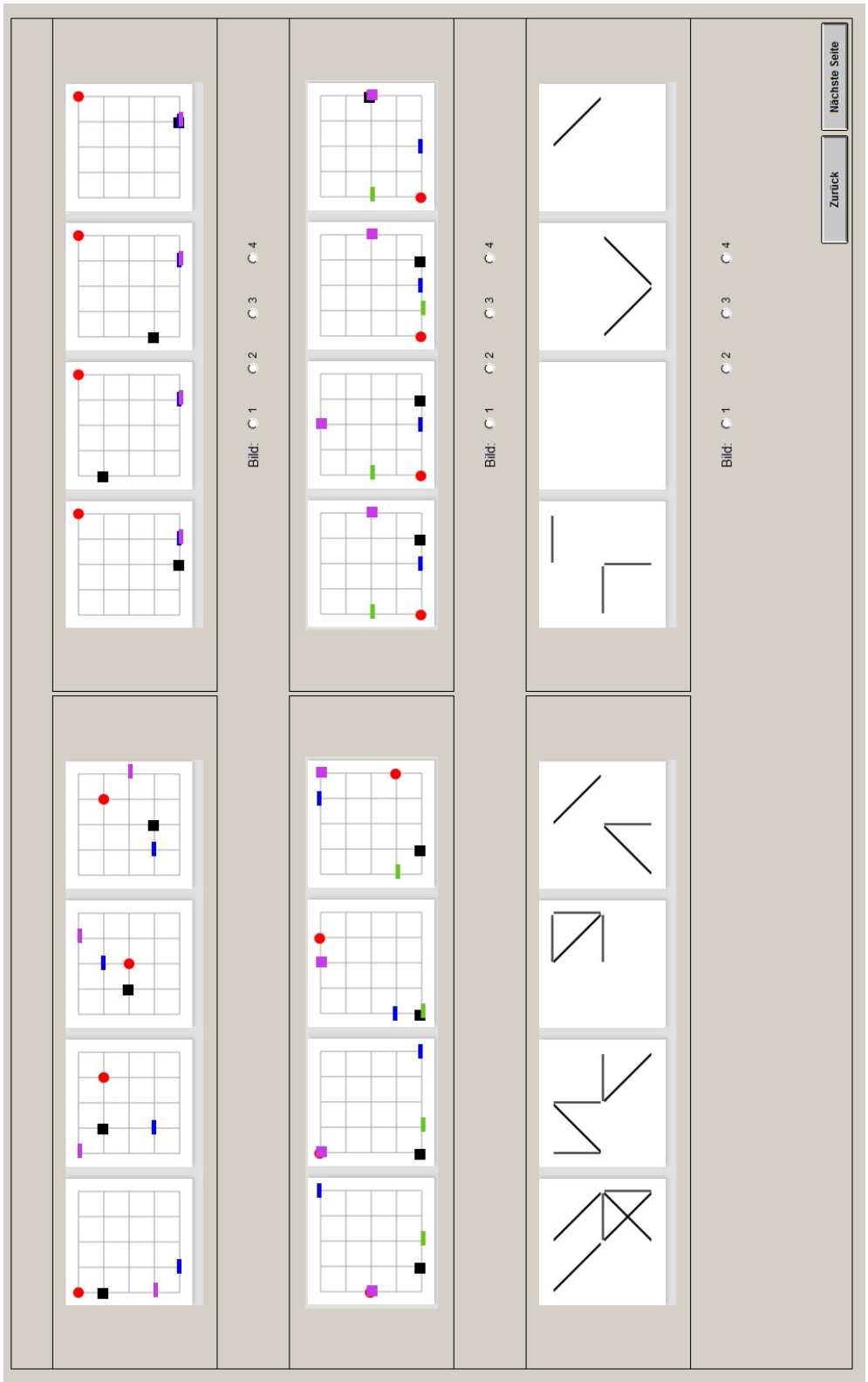
3. On what does your **income depend** in the **second revenue round**?
    a. Only on my own performance
    b. On my own performance in relation to other participants
    c. Only on the performance of the others

4. What are the options in case you want to **buy a better pen** than your **income can actually pay for?**
    a. Take out a loan
    b. Nothing
    c. Take money from other participants

5. What happens if you **cannot pay back the credit** with the earned money?
    a. I can give back the pen
    b. I have to pay the money to the experimenters
    c. The money will be deducted from my participation revenue

Material B.3.3: IQ-Quiz

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Zurück
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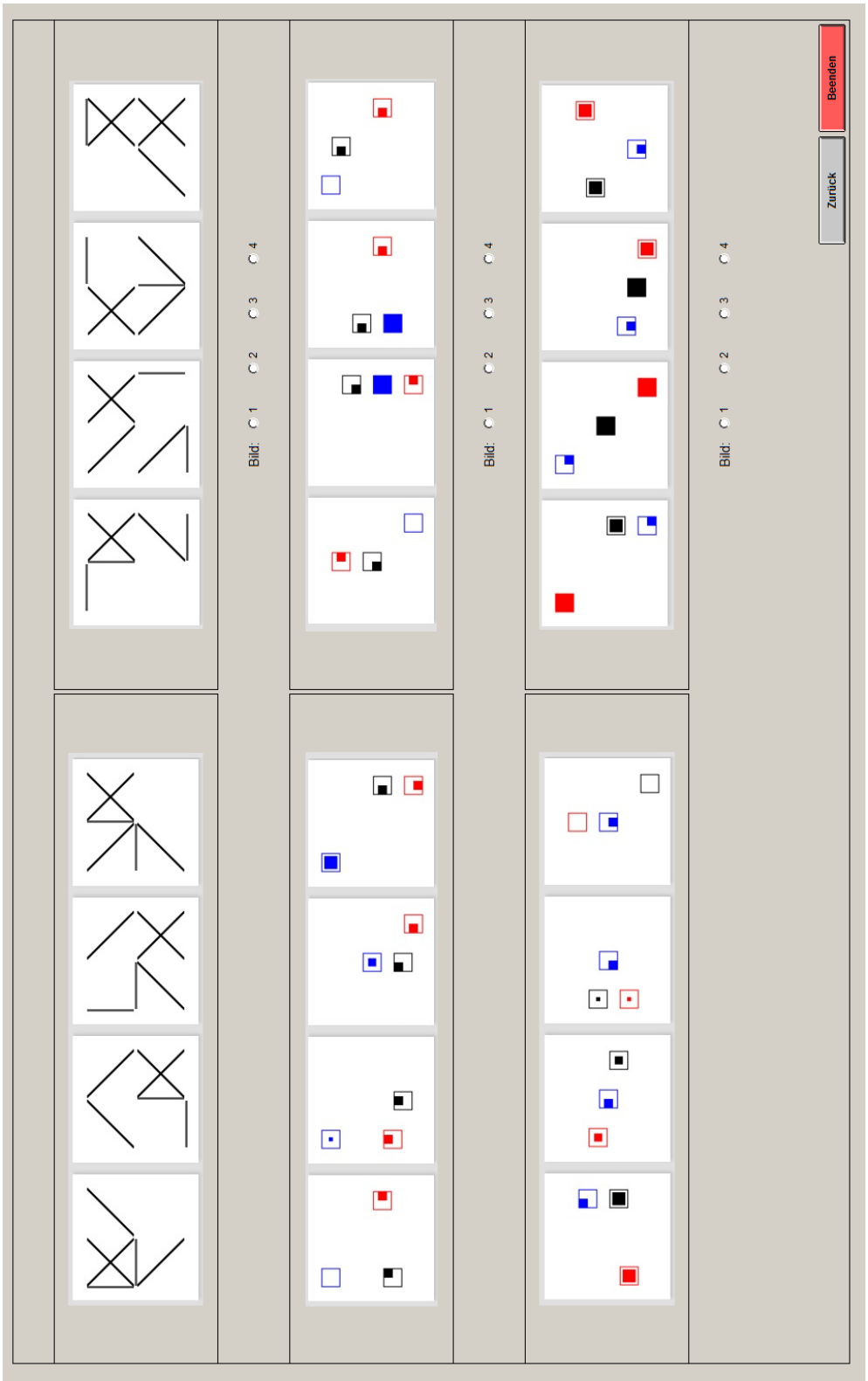
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Zurück
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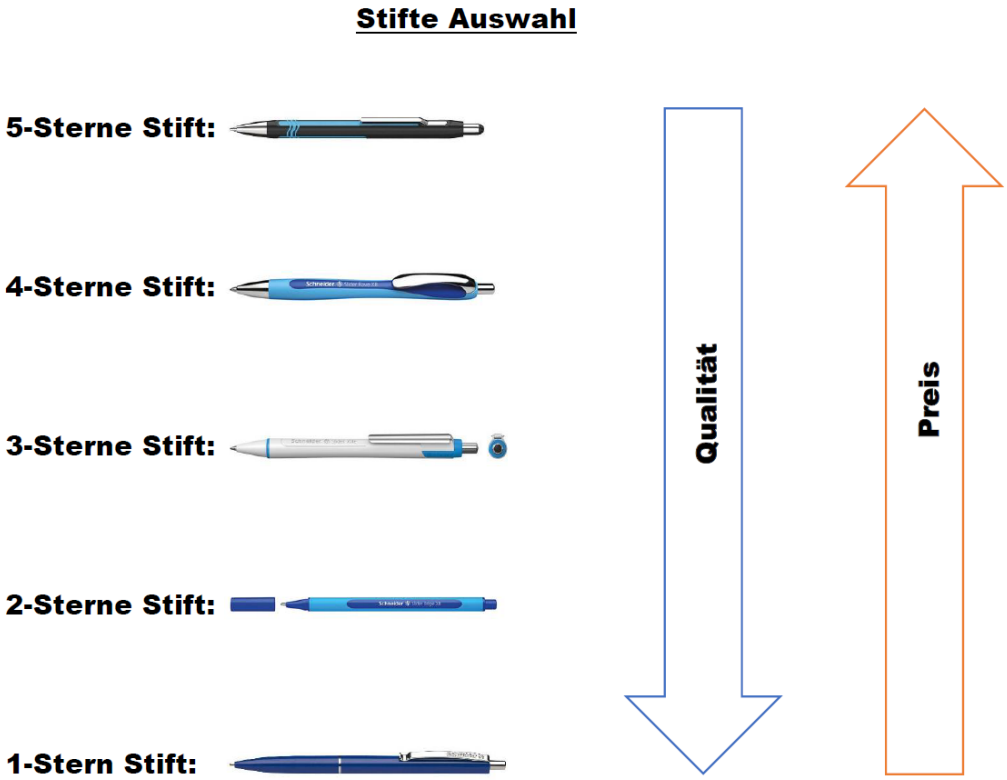
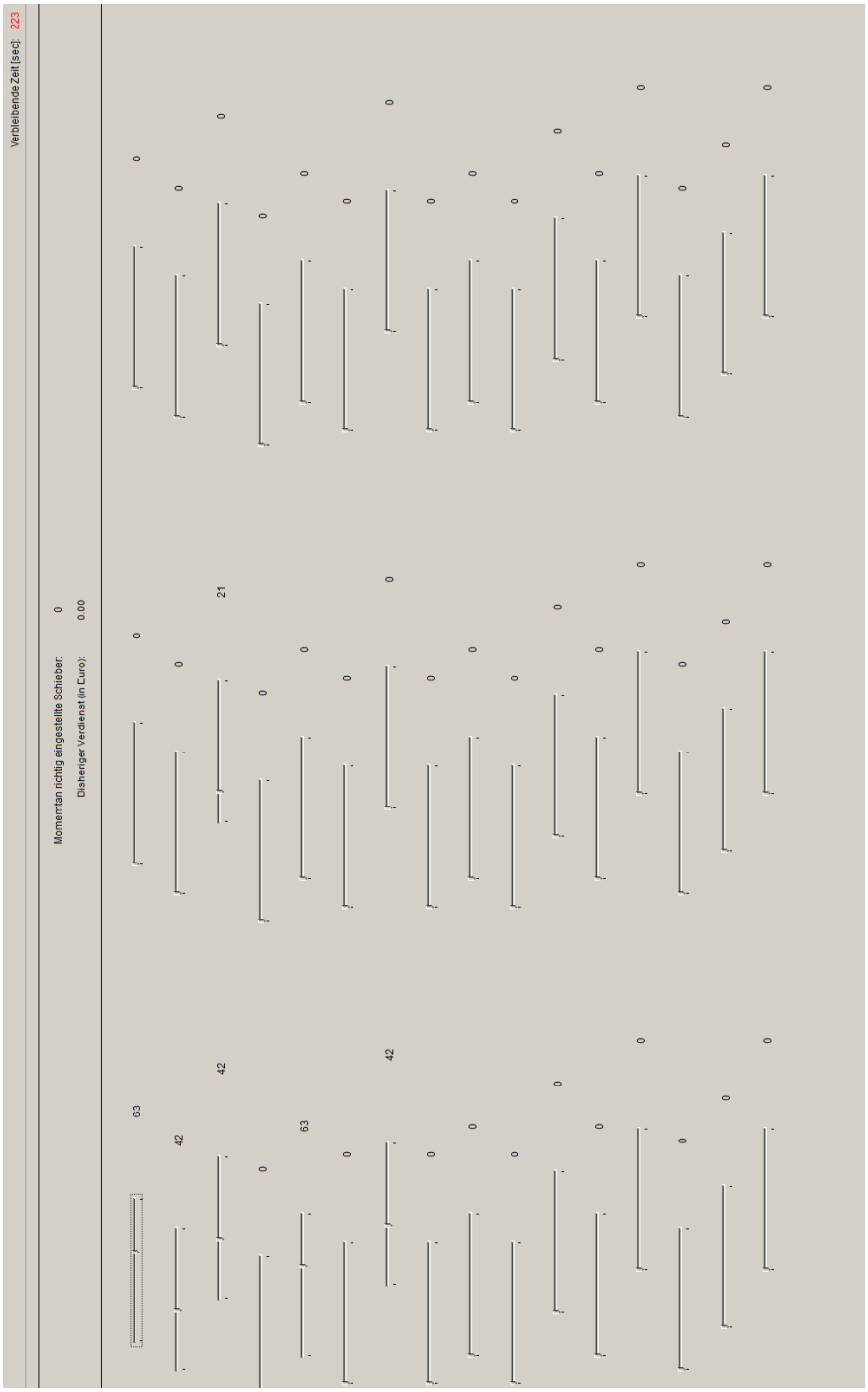
Bild: 1 2 3 4

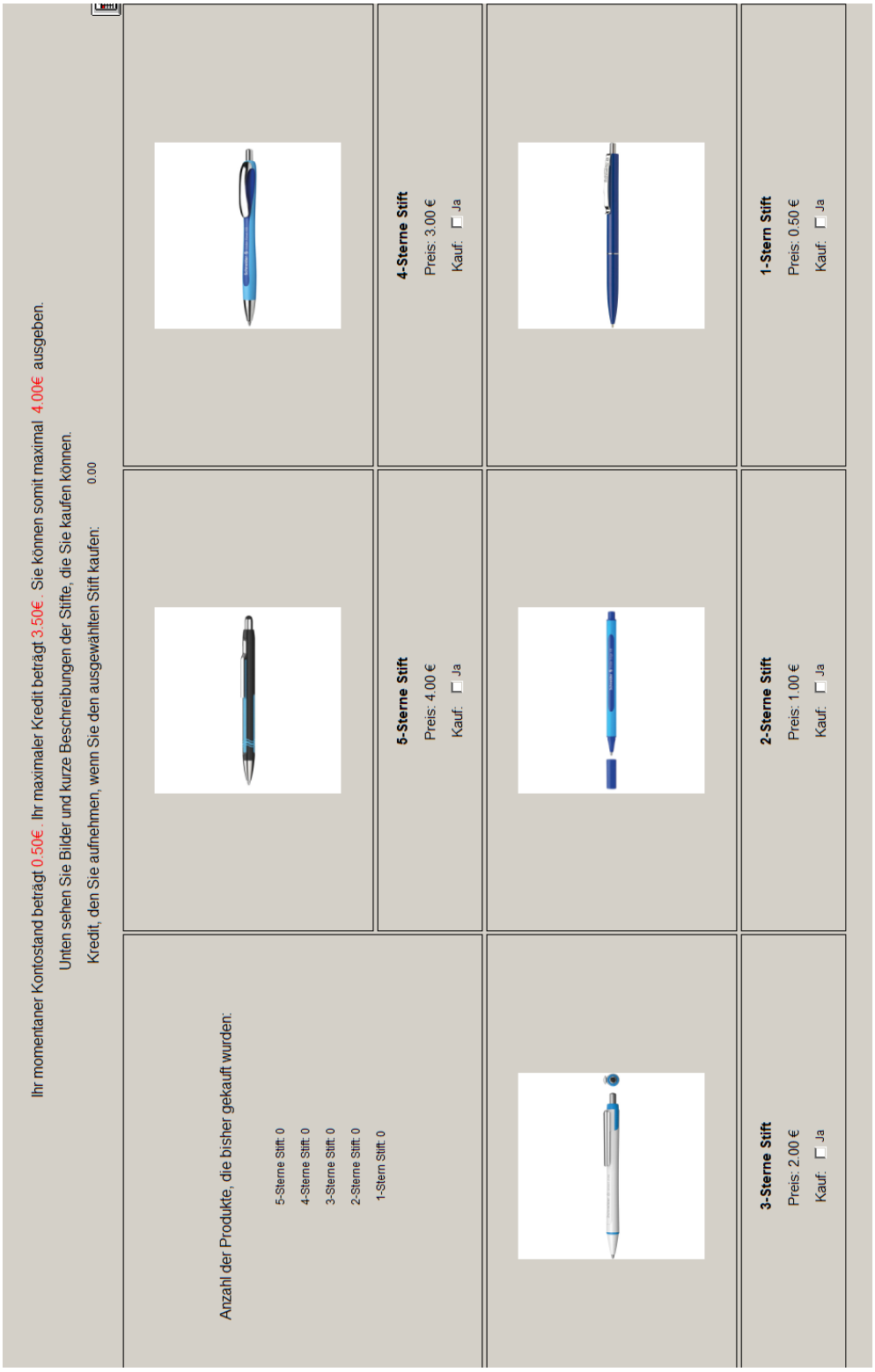
Material B.3.4: Printed Paper with Pens



Material B.3.5: Slider Task



Material B.3.6: Shopping Information Treatment



Material B.3.7: Example Products Online Survey

Produktauswahl

Als nächstes bitten wir Sie aus jeder Produktkategorie das Produkt auszuwählen, das Sie am ehesten in Ihrem alltäglichen Leben kaufen und verwenden.

Schokolade *

○ Alpia Alpenmilch 100g: Preis ca. 0,80€

○ Milka Alpenmilch 100g: Preis ca. 1,00€

○ Ritter Sport Alpenmilch 100g: Preis ca. 1,10€

○ Alnatura Bio Vollmich 100g: Preis ca. 1,30€

○ Lindor Milch 100g: Preis ca. 2,00€

Kugelschreiber *

Schneider K15: Preis ca. 0,50€

Schneider Slider Edge XB 1522: Preis ca. 1,00€

Schneider Slider Xite 1332: Preis ca. 2,00€

Schneider Slider Rave 1325: Preis ca. 3,00€

Schneider Epsilon 1386: Preis ca. 4,00€

Appendix C to accompany Chapter 4

C.1 Additional Results

Table C.1.1: Correlation Ambiguity Parameters

	Simple A.	m(0.1)	m(0.5)	m(0.9)	$AA_{0.1}$	$AA_{0.5}$	$AA_{0.9}$	b	a
Simple A.	1								
m(0.1)	0.531***	1							
m(0.5)	0.823***	0.584***	1						
m(0.9)	0.408***	0.454***	0.486***	1					
$AA_{0.1}$	−0.531***	−1	−0.584***	−0.454***	1				
$AA_{0.5}$	−0.823***	−0.584***	−1	−0.486***	0.584***	1			
$AA_{0.9}$	−0.408***	−0.454***	−0.486***	−1	0.454***	0.486***	1		
Index b	−0.710***	−0.825***	−0.835***	−0.800***	0.825***	0.835***	0.800***	1	
Index a	0.0905	0.479***	0.0621	−0.565***	−0.479***	−0.0621	0.565***	0.0245	1

Simple A. is derived from the original Ellsberg urns, where 0 means ambiguity averse, 1 means ambiguity neutral, and 2 means ambiguity seeking. *m(0.1), m(0.5), and m(0.9)* are the matching probabilities derived from the three ambiguity choice sets. $AA_{0.1}, AA_{0.5}$, *and* $AA_{0.9}$ are the differences between objective and matching probabilities-the local ambiguity attitudes. *Index b and a* are global indices for ambiguity aversion and a-insensitivity derived via linear approximation.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table C.1.2: t-tests - Groups of Interest at Baseline

	Diff. Nec.-Opp.	Diff. Rem.-Nec.	Diff. Rem.-Opp.	Diff. Ret.-Nec.	Diff. Ret.-Opp.
CE Ambiguity	31.48	−27.63	3.85	−46.46*	−14.99
CE Competition	−4.26	6.21	1.95	0.45	−3.81
Competition vs. Ambiguity	1576.99	−1051.65*	525.33	−569.15	1007.84
Others Decide	−0.02	−0.09	−0.11	−0.19*	−0.21**
Observations	35	58	65	45	52

Groups: *Diff. Nec.-Opp.*, *Diff. Rem.-Nec.*, *Diff. Rem.-Opp.*, *Diff. Ret.-Nec.* and *Diff. Ret.-Opp.* are various differences between necessity entrepreneurs, opportunity entrepreneurs, returnees, and the remaining sample. Variables: *CE Ambiguity* is the outcome for the certainty equivalent of non-strategic uncertainty, *CE Competition* is the outcome for the certainty equivalent of strategic uncertainty, *Competition vs. Ambiguity* is the outcome for the total profit in the strategic uncertainty option against non-strategic uncertainty, and *Others Decide* is the outcome for the probability of letting other decide the winning numbers instead of oneself. Positive differences indicate that value of the parameter is larger for the second group in each pair of groups.
Onesided t-tests.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table C.1.3: Correlation Ambi. Parameters and Choices in Part 4 - Control

	Index b	Index a	CE Ambiguity	CE Competition	Comp. vs. Amb.	Others Decide
Index b	1					
Index a	0.00930	1				
CE Ambiguity	0.158	0.144	1			
CE Competition	0.0217	0.0286	0.603***	1		
Comp. vs. Amb.	−0.00690	0.237**	0.433***	0.301***	1	
Others Decide	−0.0392	−0.0151	0.149	0.0641	0.0162	1

Index b and a are global indices for ambiguity aversion and a-insensitivity derived via linear approximation.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table C.1.4: Correlation Ambi. Parameters and Choices in Part 4 - Treatment

	Index b	Index a	CE Ambiguity	CE Competition	Comp. vs. Amb.	Others Decide
Index b	1					
Index a	0.0378	1				
CE Ambiguity	0.218**	0.0273	1			
CE Competition	0.325***	0.0994	0.713***	1		
Comp. vs. Amb.	0.108	0.0986	0.477***	0.463***	1	
Others Decide	−0.0292	0.0499	0.0538	0.220**	0.137	1

Index b and a are global indices for ambiguity aversion and a-insensitivity derived via linear approximation.
* p < 0.10, ** p < 0.05, *** p < 0.01

Figure C.1.1: Expected Performance in Comparison to Others by Treatment

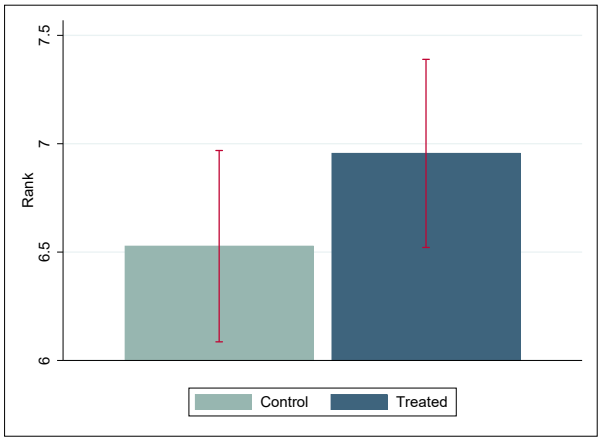


Table C.1.5: Controlling for the Order of Choices

	CE Ambiguity		CE Strategic		MP Strat.-Ambi.	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	17.297*	18.138*	6.166	6.344	794.907*	852.412*
	(11.532)	(11.875)	(11.370)	(11.353)	(518.701)	(523.377)
Order		4.054				
		(6.765)				
Order				−1.817		
				(7.054)		
Order						−526.347*
						(375.923)
Constant	90.368***	81.782***	101.218***	104.852***	1130.227***	2120.717***
	(7.745)	(16.480)	(8.131)	(17.193)	(309.767)	(804.165)
Observations	222	222	222	222	222	222

Robust S.E. in parentheses. Onesided t-tests.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table C.1.6: Controlling for Certainty about the Guess

	CE Ambiguity		CE Strategic		MP Strat.-Ambi.	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	17.297*	16.583*	6.166	5.716	794.907*	771.620*
	(11.532)	(11.440)	(11.370)	(11.345)	(518.701)	(518.037)
Certainty		−4.435**		−2.792		−144.602*
		(2.344)		(2.236)		(99.027)
Constant	90.368***	119.276***	101.218***	119.418***	1130.227***	2072.769***
	(7.745)	(18.072)	(8.131)	(17.593)	(309.767)	(791.019)
Observations	222	222	222	222	222	222

Robust S.E. in parentheses. Onesided t-tests.
* p < 0.10, ** p < 0.05, *** p < 0.01

C.2 Covariates of Ambiguity Parameters

To analyze if socio-economic or individual characteristics drive the few differences found across our groups of interest and to assess which variables are important covariates of ambiguity attitudes, we run regressions with our ambiguity aversion parameters as dependent variables. Table C.2.1 presents results for ambiguity aversion derived from the Ellsberg paradox, local ambiguity aversion, as well as index b and index a. We include the following potential covariates: sex, age, age squared, education, the Big Five personality traits, and two parameters measuring risk attitudes. The parameters for risk attitudes, derived by employing the method by Tanaka et al. (2010), measure the degree of risk aversion (*Sigma*) and probability weighting (*Alpha*), respectively.[1] The descriptions of all independent variables are in Appendix 5.5. We include occupational groups, using necessity entrepreneurs as the base group. As can be seen, results regarding the differences to other groups stay robust even when controlling for other covariates, especially those with respect to a-insensitivity. Thus, the differences between the groups cannot be explained by differences in other characteristics.

Looking at the other variables, our sample does not seem to differ much from other samples considered in the existing literature. At a first glance, it seems that we find an inconclusive effect for being male, as it is significantly positively related to ambiguity aversion, as measured by Ellsberg, but it has no effect on the matching probability of 90% and a negative effect on those for 50%. We find a similar pattern for a-insensitivity and being male. However, this finding resembles the pattern found by Borghans et al. (2009). For Dutch high-school students, they find that boys are more ambiguity averse for moderate levels of ambiguity but there is no difference between boys and girls for high levels of ambiguity. Dimmock et al. (2015) also find men to be more ambiguity averse than women. Age is significantly positively related to being more ambiguity averse, especially for moderate and high probabilities. Furthermore, the relation between age and ambiguity aversion is hump-shaped, which implies that the middle-aged are the most ambiguity averse. Surprisingly, there is only a marginal relation between risk aversion and ambiguity aversion. A-insensitivity is not at all related to risk aversion. However, as in the study of Abdellaoui et al. (2011), a-insensitivity is significantly related to inverted S-shaped probability weighting, which, intuitively, makes sense. Out of the Big Five, openness seems to be significantly negatively related to ambiguity aversion for moderate probabilities. Persons who score high on openness are described as willing to engage in new experiences and are, therefore, potentially more likely to engage in risky behaviors. Conscientiousness seems to be related to more ambiguity aversion for 50-50 probabilities and agreeableness to more ambiguity aversion for higher probabilities.

Similar to Dimmock et al. (2016), we conclude that, in general, the explanatory power of socio-economic variables for ambiguity attitudes is low. Remarkably, we find similar correlations to those of Dimmock et al. (2015) for the US-American population. Besides the difference in gender, they estimate that older persons are less ambiguity averse and that the correlation between risk and ambiguity aversion is rather low. We also cannot confirm that risk aversion and ambiguity aversion have a strong link to each other, as is shown in other studies (e.g.

[1] In the first part of the experiment, participants answered the required multiple price list choices to calculate these parameters.

Butler et al., 2014; Charness and Gneezy, 2010; Dimmock et al., 2016). Trautmann and van de Kuilen (2015) conclude that the evidence is suggestive for a positive link and that probability weighting (which is not collected in most of the studies) might serve as an mediator but that more research is necessary to clarify the empirical relationship between the two attitudes.

Table C.2.1: Socio-economic Predictors

	Ambiguity Averse	$AA_{0.1}$	$AA_{0.5}$	$AA_{0.9}$	Index b	Index a
Male	20.217***	−0.065	0.067	−0.000	0.002	0.080
	(6.875)	(0.046)	(0.043)	(0.049)	(0.078)	(0.059)
Age	4.237***	0.011	0.029***	0.025**	0.043***	0.017
	(1.388)	(0.009)	(0.009)	(0.010)	(0.015)	(0.013)
Age^2	−0.054***	−0.000	−0.000***	−0.000***	−0.001***	−0.000
	(0.017)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education	1.333	−0.007	0.017	0.003	0.009	0.013
	(2.053)	(0.013)	(0.012)	(0.014)	(0.021)	(0.018)
Sigma	−14.502*	−0.024	−0.058	−0.016	−0.065	0.011
	(7.826)	(0.041)	(0.041)	(0.047)	(0.067)	(0.067)
Alpha	−5.600	0.126	−0.044	−0.079	0.002	−0.256*
	(13.633)	(0.092)	(0.080)	(0.089)	(0.141)	(0.131)
Agreeableness	1.488	0.016	0.010	0.019*	0.030*	0.004
	(1.673)	(0.010)	(0.010)	(0.011)	(0.018)	(0.013)
Extraversion	1.191	0.004	0.005	0.006	0.010	0.002
	(1.663)	(0.011)	(0.011)	(0.011)	(0.019)	(0.011)
Conscientiousness	3.036**	0.003	0.011	0.002	0.011	−0.001
	(1.531)	(0.010)	(0.010)	(0.011)	(0.017)	(0.011)
Neuroticism	−0.812	−0.005	0.001	0.005	0.001	0.012
	(1.352)	(0.009)	(0.009)	(0.010)	(0.016)	(0.012)
Openness	−3.956***	−0.009	−0.014*	−0.008	−0.021	0.001
	(1.448)	(0.009)	(0.008)	(0.010)	(0.016)	(0.010)
Remain	14.128	−0.075	0.063	−0.016	−0.019	0.074
	(9.731)	(0.062)	(0.056)	(0.066)	(0.100)	(0.080)
Opp.	15.103	−0.057	0.102^	0.052	0.064	0.136^
	(11.524)	(0.072)	(0.063)	(0.076)	(0.115)	(0.094)
Return	3.142	−0.094^	−0.008	0.005	−0.064	0.123^
	(10.577)	(0.068)	(0.062)	(0.072)	(0.112)	(0.076)
Constant	−49.133	−0.406*	−0.608***	−0.094	−0.739**	0.389
	(31.848)	(0.208)	(0.189)	(0.221)	(0.328)	(0.294)
Observations	222	222	222	222	222	222
Adj. R-squared	0.050	-0.007	0.031	-0.014	0.000	-0.007

Variables: *Male* is an indicator for being male or female; *Age(^{2})* is the age of the participant in years (squared); *Education* is a categorical variable from 1-9, where 1 is "no degree/no education" and 9 is "doctoral degree or equivalent;" *Sigma and Alpha* are risk attitudes; *Agreeableness, Extraversion, Conscientiousness, Neuroticism, and Openness* are the Big Five personality traits; *Opp.* only includes entrepreneurs out of opportunity; *Return* represents the group of returnees; and *Remain* includes all persons who are neither entrepreneurs nor returnees. Robust S.E. in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01, ^ p < 0.10 (onesided)

C.3 Description of Variables

Groups	
Entrepreneurs (Ent.)	Participants who successfully established a business at least once including those who set up a business and have a migration history.
Necessity Entrepreneurs (Nec.)	Those entrepreneurs who set up a business and felt the most important reason was out of necessity, i.e. no other job possibilities, fear of losing their existing job, more money needed, or other reasons.
Opportunity Entrepreneurs (Opp.)	Those entrepreneurs who set up a business and felt the most important reason was because they had the opportunity, i.e. ideal form of work to be self-employed, opportunity to be in charge, opportunity to earn more money, or other reasons.
Returnees (Return)	Participants who spent at least three consecutive months abroad in the last six years for non-recreational purposes and who never set up a business.
Remaining (Remain)	Participants who are neither entrepreneurs nor returnees.

Independent Variables	
Kosovo	Dummy that takes the value 1 if the person participated in a session in Kosovo and 0 if the person participated in a session in Albania.
Male	Dummy that takes the value 1 if the participant is male and 0 if the participant is female.
Age(^{2})	Age (squared) of the participant in years.
Education	Variable with the following options: 1-“No degree/no education,” 2-“Primary education,” 3-“Lower secondary education’,’ 4-“Upper secondary education,” 5-“Post-secondary non-tertiary education,” 6-“Short-cycle tertiary education (no university diploma),” 7-“Bachelor or equivalent”, 8-“Master or equivalent,” 9-‘ ´Doctoral or equivalent”.

Dummy Working Dummy that takes the value 1 if the participant worked in the last seven days and 0 if not.

Work Status Employment status for participants who work. Categorical variable with the following options: 1-"Paid full-time employee," 2-"Paid part-time employee," 3-"Paid intern/Paid apprentice," 4-"Employer," 5-"Self-employed," 6-"Unpaid intern/Unpaid apprentice," and 7-"Unpaid worker in household business".

Sigma A measure for risk aversion that estimates the concavity of the value function. Estimated with the method from Tanaka et al. (2010).

Alpha A measure for probability weighting that equals 1 if the weighting function is linear, < 1 if it is inverted S-shaped, and > 1 if the function is S-shaped. Estimated with the method from Tanaka et al. (2010).

Agreeableness One of the Big Five personality traits that is measured with two different items. Participants answer on a scale from 1-"Strongly disagree" to 7-"Strongly Agree" if the following traits apply to them: 1. "Critical, quarrelsome" and 2. "Sympathetic, warm".

Extraversion One of the Big Five personality traits that is measured with two different items. Participants answer on a scale from 1-"Strongly disagree" to 7-"Strongly Agree" if the following traits apply to them: 1. "Extraverted, enthusiastic" and 2. "Reserved, quiet".

Conscientiousness One of the Big Five personality traits that is measured with two different items. Participants answer on a scale from 1-"Strongly disagree" to 7-"Strongly Agree" if the following traits apply to them: 1. "Dependable, self-disciplined" and 2. "Disorganized, careless".

Neuroticism One of the Big Five personality traits that is measured with two different items. Participants answer on a scale from 1-"Strongly disagree" to 7-"Strongly Agree" if the following traits apply to them: 1. "Anxious, easily upset" and 2. "Calm, emotionally stable".

Openness One of the Big Five personality traits that is measured with two different items. Participants answer on a scale from 1-"Strongly disagree" to 7-"Strongly Agree" if the following traits apply to them: 1. "Open to new experiences, complex" and 2. "Conventional, uncreative".

C.4 Instructions

Material C.4.1: Welcome Script

Choosing Risk Interactive Classroom Sessions

Instructions to be given to participants as a hard copy and to be read aloud together

**********[BEGINNING OF WRITTEN INSTRUCTIONS]**********

General Instructions

Welcome and thank you for participating in this interactive session today. You have been invited because you completed a survey last year for a project titled "Exploring recent trends in economic migration". You agreed to be contacted for further participation in our research. This interactive session is for the purpose of research on economic decision making and risk. For this session, it does not matter whether you have any migration in your past or future. We want to know preferences from a wide variety of different people here in *[INSERT ALBANIA OR KOSOVO]*.

****

[Alternative wording, in case we need to recruit people who did not take the survey.]

You have been invited to participate in this interactive session for the purpose of research on economic decision making and risk. It is one element of a wider research project titled "Exploring recent trends in economic migration". There has also been a household survey for this project, that some of you have answered already. For this session, it does not matter whether you have any migration in your past or future. We want to know preferences from a wide variety of different people here in *[INSERT ALBANIA OR KOSOVO]*.

****

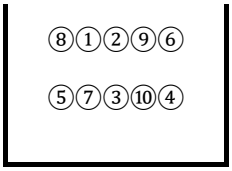
We will give each person a lump sum of *[INSERT EUR OR ALBANIAN LEK AMOUNT]* for coming today. If you follow the instructions carefully, you can also earn a good amount of additional money. You will be paid in cash in private at the end of the session. It is important that you do not talk, or try to communicate, with other participants during the session. Please also put your mobile phones on silent and refrain from using them during the session. If you have any questions once the session has started, please raise your hand and a moderator will come over to where you are seated to answer your question in private.

This interactive session consists of two parts. During the first part you will be asked to make a number of decisions that involve risk. "Risk" means that the effects of a decision cannot be known for certain at the time the decision is made, and the effects may be better or worse due to chance.

To give you an idea of the types of risky scenarios you will see, consider this bucket with 10 balls, numbered ① ② ③ ④ ⑤ ⑥ ⑦ ⑧ ⑨ and ⑩.

[The moderator shows an actual bucket, which is filled with numbered ping pong balls.]

It is like the one pictured below.



We will now do a hypothetical example. We will talk about points but the points will not translate to money in this example.

Here is the example. I will give you the opportunity to draw a ball form the bucket. If you draw a ① ② or ③, I will give you 300 points. If you draw any other number you get 0 points.

But suppose you don't like this scenario. I will give you a second option. Instead of trying to get 300 points by drawing a ball from the bucket, which could also result in 0 points, you can choose to get a flat pay-out of 80 points. If you want that option, you would get the 80 points for sure and there would be no draw from the bucket.

Do you prefer:

- Get 80 points for sure OR
- Draw a ball from the bucket for the chance to get 300 points. Get 300 points if a ① ② or ③ is drawn. Get 0 points if any other ball is drawn.

[The moderator allows one participant to draw a ball from the physical bucket and announces the result.]

During the session we will let the computer perform the draws, so that each person can get draws that only apply to them. This bucket is for illustration only, so that you understand how the computer will make a fair draw based on your choices. You will not actually see the bucket illustration – the computer will do this in the background.

You will not know which ball the computer will draw – it is random. You only know how many balls in the bucket indicate the high and low pay-outs.

Please raise your hand now if you have any question.

In summary, you will be asked to make choices about your earnings when you do not know for sure what the outcome of the choice will be.

For some of your decisions, the chances of earning a good amount will also depend on the decisions that others make. How you can earn money from your decisions will be described in detail each time you begin a new task. During the second part of the session you will be asked to complete a short survey.

By following instructions carefully and completing tasks to the best of your ability you can earn a good amount of money.

How earnings are determined

In addition to the *[INSERT EUR OR ALBANIAN LEK AMOUNT]* you will receive for coming today, you can earn money by the decisions you make. For each decision, you will earn points. The points will be converted to cash. For every *[INSERT NUMBER OF POINTS]* points you earn you will receive *[INSERT EUR OR ALBANIAN LEK AMOUNT]* in cash at the end of the session.

To earn points, you will be asked to choose between different earning options, each of which involves different amounts of possible earnings and different amounts of risk. Options with more risk have higher potential earnings, but there is also a chance that you get a very low amount. Options with low risk have lower potential earnings, but higher chance of getting the money. Once you make your choices, the computer will determine the outcome.

All earnings will be paid out in cash in private at the end of the session today. You will be asked to sign a receipt of payment, acknowledging that you have received the earnings for participation in this interactive session only.

If you have any questions at this time, please raise your hand.

Informed consent

Before the session begins, we must obtain permission from each of you to use your replies today for this research. This is called “obtaining informed consent”. We have prepared a form for you to read and sign for this purpose. It is on the desk in front of you, with the heading “Consent Form”.

This form describes the research, how the information we collect will be used and how we will protect your anonymity. I will give you a few minutes to read the form. Please raise your hand if you have a question and I will come to you. Once we have collected all the signed consent forms, we will proceed.

[Spoken only.]

We will now hand you a tablet which you will use to complete the tasks during this interactive session. Please wake up your tablet and a welcome screen will appear. Please raise your hand if the tablet does not display a welcome message.

[Spoken only.]

You will also receive an ID card. The ID card is to preserve your anonymity. Please do not share your ID with any other participant.

**********[END OF WRITTEN INSTRUCTIONS]**********

Material C.4.2: Instructions Ambiguity Aversion Parameters

**********[BEGINNING OF PART II]**********

[SCREEN 2.1 - to be read out loud.]

Header: ***Part II, introduction***

This is Part II. Tasks in Part II will also entail deciding between different earnings opportunities, but the options are different from those in Part I.

Like before, there will be two possible earnings opportunities, and both opportunities will be risky.

Unlike before, you will not always know the exact chances for getting points.

Please pay careful attention to the example of the task, which is on the next few pages.

Click “Next” to proceed.

[SCREEN 2.2 - to be read out loud.]

Header: ***Part II, example***

Please imagine the following two earnings opportunities, where you can get points by drawing a ball from a bucket:

There are two buckets. Each bucket has 100 balls. Each ball is either blue or orange. The two buckets have different combinations of orange and blue balls.

Earnings are determined by first choosing a bucket, then drawing a ball from the chosen bucket. Blue and orange balls have different values. If the drawn ball is orange you will receive 0 points and if the drawn ball is blue you will receive 100 points.

The two buckets are:

Bucket A: you don't know how many balls are orange and how many balls are blue. Put differently, if you have to draw a ball from the bucket, you do not know the probability of drawing an orange or a blue ball.

Bucket B: you know the exact number of orange and blue balls in this bucket. In other words, if you have to draw a ball from the bucket, you know the exact probability of drawing an orange or a blue ball.

The two buckets are pictured on the next screen.

Click “Next” to continue.

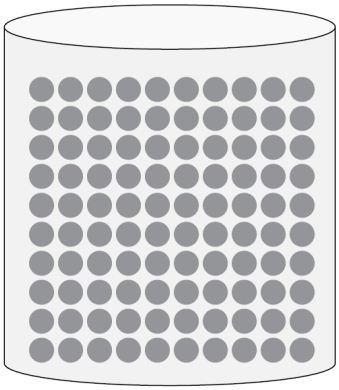
[SCREEN 2.3 - to be read out loud.]

Header: ***Part II, example***

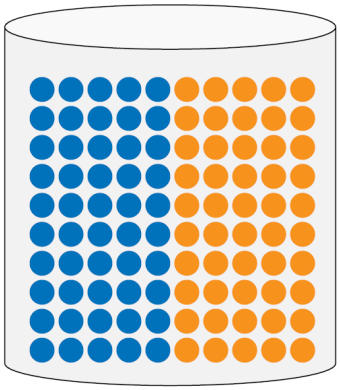
The images below show the two buckets. **Each bucket contains some orange and some blue balls.** To illustrate that Bucket A could have any number of blue or orange balls, these balls are coloured in grey here. But each ball is either orange or blue for sure.

The picture of B has exactly 50 orange and 50 blue balls. This illustrates that you know the content of Bucket B.

Although the balls are ordered by colour in the picture, you can imagine that the buckets will be shaken such that all balls are mingled. The picture is designed in this specific way to help you recognize how many balls of each colour are in the bucket.



Unknown chance for a blue ball



50% chance for a blue ball

Click “Next” to proceed.

[SCREEN 2.4 - to be read out loud.]

Header: ***Part II, example continued***

Your task is to choose which bucket is used for the draw. Only one ball will be drawn.

No matter which bucket you choose, if the drawn ball is orange you will receive 0 points and if the drawn ball is blue you will receive 100 points.

- If you indicate a preference for Bucket A: a ball is drawn from Bucket A and you will receive 100 points if the ball is blue and 0 points if the ball is orange.
- If you indicate a preference for Bucket B: a ball is drawn from Bucket B and you will receive 100 points if the ball is blue and 0 points if the ball is orange.

[SCREEN 2.5 - to be read out loud.]

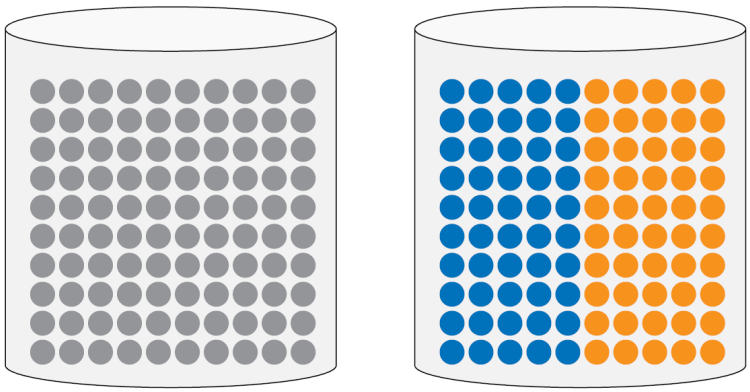
Try selecting an option. The two small circles below the buckets are for you to indicate your preference, the left circle corresponds to bucket A, while the right circle corresponds to bucket B.

Make sure you understand how to enter the choice or change the choice.

Do you prefer:

- draw the ball from **Bucket A** (where you do not know your chances for getting a blue ball)

- draw the ball from **Bucket B** (where you know the chance of a blue ball is 50%).



Unknown chance for a blue ball 50% chance for a blue ball

Once you are sure of your choice, please click “Next”.

[SCREEN 2.6 - to be read out loud.]

Header: ***Part II, instructions***

You will be asked to complete tasks like this example a number of times. We will present the choices in sets. There are 3 sets in Part II. Each set entails 4 tasks.

There will always be two options to choose from. In one option you will *not* know the chances of getting the points. For the other option you *will know* the exact chances of getting the points.

We will provide you with further instructions before each set, if required.

Click “Next” to proceed.

[SCREEN 2.7 - to be read out loud.]

Header: ***Part II, how earnings are determined***

In each set of tasks you complete, one of the tasks will be randomly selected by the computer to determine payment for that set. The ball will be drawn from the bucket you chose in that task, and you will get the points corresponding to the colour ball that was drawn.

You will not learn your earnings from individual tasks, but will see them reflected in the point totals at the end of the session today.

Consider each task carefully as all tasks are equally likely to be selected for payment.

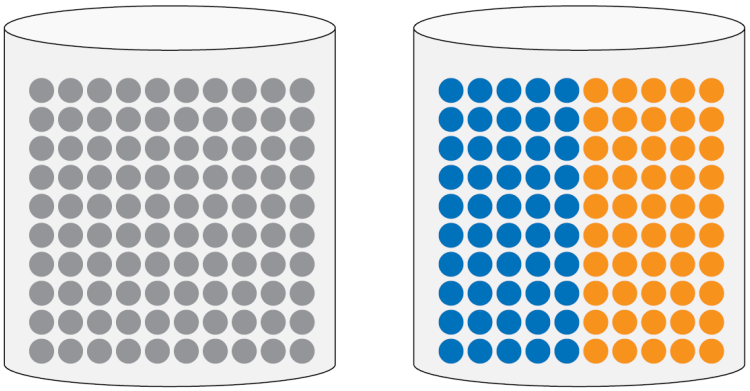
Click “Next” to proceed.

[SCREEN 2.8 - to be read out loud.]

Header: ***Part II, comprehension questions***

Before we start with the first set we just want to make sure that our instructions on the tasks were clear. Therefore, we ask you to answer the following comprehension questions:

Consider the two buckets, Bucket A and B. Bucket A contains orange and blue balls, but the exact composition is unknown. Bucket B contains exactly **50** orange balls and **50** blue balls.



Please indicate if the following statement is true or false: "There are more blue balls in Bucket A than in Bucket B."

1. True
2. False
3. Cannot be known

Please indicate if the following statement is true or false: "It is more likely to draw a blue ball from Bucket A than it is from Bucket B."

1. True
2. False
3. Cannot be known

Click "Next" to proceed.

[SCREEN 2.9 - TO BE SEEN if at least one answer was incorrect]

Header: ***Part II, answers***

Answer to the first question:

In fact, it cannot be known which bucket contains more blue balls, because the composition of orange and blue balls in Bucket A is unknown.

Answer to the second question:

In fact, it cannot be known whether it is more likely to draw a blue ball from Bucket A than from Bucket B, because the composition of orange and blue balls in Bucket A is unknown.

If you have a question regarding these answer, please raise your hand now.

Otherwise, click “Next” to proceed.

[INSERT WAITING SCREEN]

[SCREEN 2.10 - to be read out loud.]

Header: ***Part II, any questions?***

If you have any questions at this time, please raise your hand. We will answer questions for everyone to hear before you begin the tasks.

Please remember, once you begin the tasks, if you have any questions quietly raise your hand and a moderator will come and answer your questions in private. It is important you do not talk to any other participants from this point onwards.

Click "Next" to proceed.

Material C.4.3: Instructions Competence Treatment

[SCREEN 3.11 - to be read out loud.]

Header: ***Part III, Set 2, instructions***

For the next several tasks, you will make decisions between two options. One option in each pair will involve entering competition. The differences between the options will change from one task to the next, so please consider each choice carefully. By offering you many different tasks, we hope to learn more about how you make decisions that involve risk and competition.

[SCREEN 3.12 - to be read out loud.]

Header: ***Part III, competition instructions***

In this activity, competing can be thought of as obtaining a share of a limited amount of profits. You do not have to win a contest. Just by entering the competition you get a share.

However, the size of your share depends on the number of competitors you face.

Click "Next".

[SCREEN 3.13 - to be read out loud.]

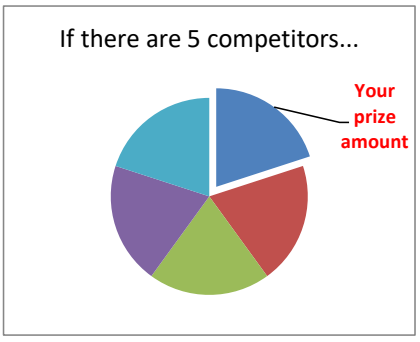
Header: ***Part III, competition instructions***

How will competitors impact the points I can earn?

If you chose to enter the competition, your earnings will be determined by splitting the total available among you and all the other competitors in your group. The amount is shared equally.

For example, suppose there are 300 points available for competition and three people are in the competition. Each person would get 100 points.

If there are few competitors, the prize per person is high. If there are many competitors, the prize per person is low. The charts below provide an example.



Click "Next".

[SCREEN 3.14 - to be read out loud.]

Header: ***Part III, competition instructions***

If you choose the Competition option and there are no other competitors, you would earn the entirety of the total points available.

If you choose the Competition option and everyone else in the group is also a competitor, you get one fifth of the available points.

If you do not choose to compete, your earnings are not dependent on the number of competitors in your group.

Please raise your hand if you have a question.

[SCREEN 3.15 - to be read out loud.]

Header: ***Part III, competition instructions***

How many competitors could I face?

You will be randomly assigned to a group. If you chose to compete, your competitors will come from this group. If you do not choose to compete, you will not face competitors, and the group will not matter.

Your group will always has the same people in it. It will consist of you, and 4 other people who did a session already. So they are not in the room today.

Each other person in your group will either be a "competitor" or "not a competitor". The others already did their session. They are "Competitors" if they chose competition more than they chose other options.

You will not know how many people in your group are competitors. So you will not know your exact prize amount if you chose the competition option. In all tasks, the number of competitors you would face is the same.

[SCREEN 3.16 - to be read out loud.]

Header: ***Part III, competition instructions***

To indicate the competition option in each task, we will use the following diagram. The diagram shows the highest and lowest possible points for the competition option.

300 ⇨ 👥 ???
A share of 300 points. The size of your share depends on competitors the highest possible points for you are 300, the lowest possible points for you are 60

The image 👥 ??? indicates your competitors. **There can be 1,2,3 or 4 competitors**, but this image will always be the same. There are ??? because you do not know how many competitors you face.

Click "next"

[SCREEN 3.17 - to be read out loud.]

Header: ***Part III, competition instructions***

Before we start with the next set we want to make sure that our instructions about the group were clear. Therefore, please answer the following questions:

How many people will be in your group, besides you?

_____

True or False: I will not know how many competitors I will face in the competition options.

Please indicate how many competitors you think will be in your group. Exclude yourself from your estimations.

Competitors: _____

How certain are you about your estimation? Please choose a number on a scale from 1 to 10. 1 means you are not certain at all about your guess, that you picked the number more or less randomly. 10 means you are very certain about your guess, that you think you understand well people's preference for competition.

Please click "Next" to proceed.

Material C.4.4: Instructions Choices under Uncertainty

**********[BEGINNING OF PART IV]**********

[SCREEN 4.1 - to be read out loud.]

Header: ***Part IV, introduction***

This is Part IV. Tasks in Part IV will again entail deciding between different earnings opportunities. Your task will be to decide whether you prefer the option on the left side of the screen or if you prefer the option on the right of the screen.

The opportunities differ in whether you know the risk, whether others' choices affect what you can earn and what the possible earnings are. These tasks will look similar to the tasks you considered in Part III. But they are not the same.

There are 4 sets in Part IV. Each set compares different scenarios. We will provide instructions at the beginning of each set. Set 1-3 have four tasks each. The fourth set has 3 tasks.

Click "Next" to proceed.

[SCREEN 4.2 - to be read out loud.]

Header: ***Part IV, instructions***

For all tasks in Part IV you are again grouped with other persons. These are not the same persons as before. The group consists of yourself and 4 new and randomly selected people who are your potential competitors if you chose to compete. These are people like you, who completed their session already. They are not in the room today.

Click "Next" to proceed.

[SCREEN 4.3 - to be read out loud.]

Header: ***Part IV, competitors***

As in Part III, the competition preferences of your group members may matter for some tasks and not other tasks. Since your group members completed their own session already, we know whether they prefer to compete or prefer not to compete. These potential competitors will impact your earnings only if you chose to compete yourself. You do not know the number of competitors. You do know that this number does not change from one decision to the next. You will always face the same number of competitors at any point you chose to compete.

We will always tell you when the choices of your group members might matter for your payment.

Choices that persons outside your group make can never influence your earnings.

Click "Next" to learn about the earnings options.

[SCREEN 4.4 - to be read out loud.]

Header: ***Part IV, how earnings are determined***

In each set you complete, one of tasks in that set will be randomly selected by the computer to determine payment for that set.

As with Parts I-III, once the computer selects which task is used to determine payment, the computer will calculate how many points you earn depending on the choice you made.

You will not learn your earnings from individual tasks, but will see them reflected in the point totals at the end of the session today.

Consider each task carefully as all tasks are equally likely to be selected for payment.

Click "Next" to proceed.

[SCREEN 4.5 - to be read out loud.]

Header: ***Part IV, any questions?***

If you have any questions at this time, please raise your hand. We will answer questions for everyone to hear before you begin the tasks.

Please remember, once you begin the tasks, if you have any questions quietly raise your hand and a moderator will come and answer your questions in private. It is important you do not talk to any other participants from this point onwards.

Click "Next" to proceed.

[Set 1 – 3 will appear in random order]

Material C.4.5: Instructions Fourth Choice

[SCREEN 4.27]

Header: ***Part IV, fourth set, instructions***

This is the last set of tasks that you will complete for earning points. At most you will do three tasks in this set. This set is similar to Part I.

Consider again the bucket with 10 balls, numbered ① ② ③ ④ ⑤ ⑥ ⑦ ⑧ ⑨ and ⑩. The two scenarios below assign different balls to indicate a "high" and a "low" earning:

	Scenario A		**Scenario B**	
	150 points	if ①②③④⑤	150 points	if ⑥⑦⑧⑨⑩
	0 points	if ⑥⑦⑧⑨⑩	0 points	if ①②③④⑤

In words:

- If you indicate a preference for Scenario A you will receive **150** points if ball ① ② ③ ④ or ⑤ is drawn from the bucket and **0** points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn from the bucket.
- If you indicate a preference for Scenario B you will receive **0** points if ball ① ② ③ ④ or ⑤ is drawn from the bucket and **150** points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn from the bucket.

This means, you can decide which balls are the "winning" balls. You cannot change the risk, there are always 5 balls that give you 150 points and 5 balls that give you 0 points. You just decide which balls are the winning balls for you. Each person has their own choice and their own draw from the bucket.

[NEXT SCREEN]

In addition, for this set you are no longer in the group with 4 other people. Instead you are randomly **matched to one single person in this room** which will be your partner for this set.

You will have the option to ask your partner to choose the winning balls for your draw.

Your partner faces the same decision as you and can decide that you have to choose the winning balls for her/him.

At the end of this set, you and your partner will both be informed whether you or your partner chose the winning balls for you, and which balls end up being the winning ones for you. You will also both learn the same information about the winning balls for your partner.

If you have any questions at this time, please raise your hand. We will answer questions for everyone to hear before you begin the tasks.

Please remember, once you begin the tasks, if you have any questions quietly raise your hand and a moderator will come and answer your questions in private. It is important you do not talk to any other participants from this point onwards.

Click "Next" to proceed.

[SCREEN 4.28]

Header: ***Part IV, fourth set, first task***

Who should decide which balls are the winning balls?

Myself Another Participant

[SCREEN 4.29]

Header: ***Part IV, fourth set, second task***

[FORMAT 1 – TO BE SEEN if participant clicked "Myself"]

You decided that you will decide on your own which balls are "winning balls"..

The two small circles situated between the scenarios are for you to indicate your preference, the left circle corresponds to Scenario A, while the right circle corresponds to Scenario B.

	Scenario A		**Scenario B**	
	150 points	if ①②③④⑤	150 points	if ⑥⑦⑧⑨⑩
	0 points	if ⑥⑦⑧⑨⑩	0 points	if ①②③④⑤

[FORMAT 2 – TO BE SEEN if participant clicked "Another Participant"]

You decided that another participant will decide for you which balls are "winning balls".

Click "Next" to proceed.

[SCREEN 4.30 – only if participant has to decide for another one]

Header: ***Part IV, fourth set, third task***

Your partner wants you to decide for him/her which balls are "winning balls". Which scenario do you choose? Notice that your partner will be informed what your decision was.

The two small circles situated between the scenarios are for you to indicate your preference, the left circle corresponds to Scenario A, while the right circle corresponds to Scenario B.

	Scenario A		**Scenario B**	
	150 points	if ①②③④⑤	150 points	if ⑥⑦⑧⑨⑩
	0 points	if ⑥⑦⑧⑨⑩	0 points	if ①②③④⑤

[SCREEN 4.31]

Header: ***Part IV, fourth set, summary***

[FORMAT 1 – TO BE SEEN if both clicked "Myself"]

You decided on your own that you receive 150 points (0 points) if ball ① ② ③ ④ or ⑤ is drawn and 0 points (150) points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn.

[FORMAT 2 – TO BE SEEN if participant clicked "Myself" and partner "Another participant"]

You decided on your own that you receive 150 points (0 points) if ball ① ② ③ ④ or ⑤ is drawn and 0 points (150) points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn.

Furthermore, you decided for your partner to receive 150 points (0 points) if ball ① ② ③ ④ or ⑤ is drawn and 0 points (150) points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn.

[FORMAT 3 – TO BE SEEN if participant clicked "Another participant" and partner "Myself"]

Your partner decided that you receive 150 points (0 points) if ball ① ② ③ ④ or ⑤ is drawn and 0 points (150) points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn.

[FORMAT 4 – TO BE SEEN if both clicked "Another participant"]

Your partner decided that you receive 150 points (0 points) if ball ① ② ③ ④ or ⑤ is drawn and 0 points (150) points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn.

Furthermore, you decided for your partner to receive 150 points (0 points) if ball ① ② ③ ④ or ⑤ is drawn and 0 points (150) points if ball ⑥ ⑦ ⑧ ⑨ ⑩ is drawn.

Click "Next" to proceed.

**********[END OF PART IV]**********

Appendix D to accompany Chapter 5

D.1 Survey Appendix

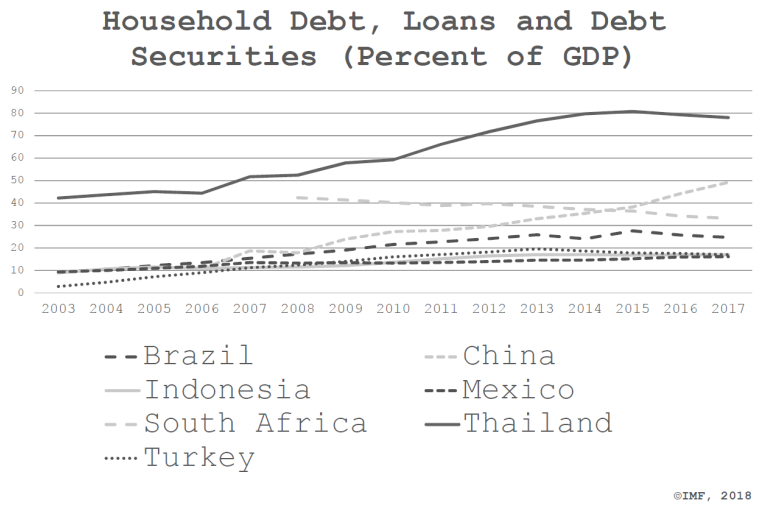


Figure D.1.1: Household Debt to GDP Ratio, Selected Emerging Markets

Table D.1.1: Correlation Matrix - Over-Indebtedness Variables

	Obj. OI-Index	DSR > 0.4	RDSR > 0.4	Holds > 2 Loans	Paid Late/ Def.	Subj. OI-Index	Debt Pos.	Diff. Pay Debt
Obj. OI-Index	1							
DSR > 0.4	0.733***	1						
RDSR > 0.4	0.771***	0.481***	1					
> 2 Loans	0.725***	0.426***	0.430***	1				
Paid Late/Def.	0.529***	0.111***	0.212***	0.141***	1			
Subj. OI-Index	0.458***	0.193***	0.347***	0.310***	0.417***	1		
Debt Position	0.485***	0.250***	0.439***	0.348***	0.302***	0.763***	1	
Diff. Pay Debt	0.298***	0.0922**	0.164***	0.169***	0.398***	0.749***	0.371***	1
Sacrifice Index	0.240***	0.0881**	0.169***	0.174***	0.233***	0.728***	0.330***	0.305***

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. The objective and subjective over-indebtedness indices are standardized with mean zero and standard deviation of one.

Table D.1.2: Subsample Probability Question: Objective OI-Indicators

	Obj. Index	DSR > 0.4	RDSR > 0.4	Paid Late	> 2 Loans
	(1)	(2)	(3)	(4)	(5)
Very Negative	−0.088	−0.067	−0.024	−0.003	−0.008
	(0.166)	(0.052)	(0.093)	(0.043)	(0.061)
Negative	0.061	−0.071	0.075	0.079**	−0.009
	(0.178)	(0.064)	(0.075)	(0.038)	(0.066)
Neutral	0.109	0.010	0.033	0.090	−0.014
	(0.196)	(0.076)	(0.066)	(0.060)	(0.068)
Positive	0.373**	0.105**	0.218***	0.141***	−0.025
	(0.137)	(0.047)	(0.063)	(0.043)	(0.058)
Constant	−1.978**	−0.103	−0.914***	−0.008	−0.448
	(0.845)	(0.315)	(0.316)	(0.303)	(0.383)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	525	525	525	522	525
Adj. R-squared	0.092	0.054	0.124	0.044	0.039

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.3: Subsample Probability Question: Subjective OI-Indicators

	Subj. Index	Debt Position	Diff. Pay off Debt	Sacrifice Index
	(1)	(2)	(3)	(4)
Very Negative	0.210	−0.003	0.059	0.282**
	(0.131)	(0.115)	(0.047)	(0.106)
Negative	0.124	0.044	0.012	0.207
	(0.118)	(0.135)	(0.027)	(0.154)
Neutral	0.019	0.026	0.017	−0.073
	(0.115)	(0.127)	(0.024)	(0.094)
Positive	0.343***	0.213**	0.057**	0.351***
	(0.092)	(0.083)	(0.025)	(0.120)
Constant	−0.872	−1.816**	0.059	0.154
	(0.829)	(0.726)	(0.181)	(0.688)
Controls	Yes	Yes	Yes	Yes
Observations	525	525	523	525
Adj. R-squared	0.109	0.076	0.055	0.119

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.4: Subsample Financial Decision Makers: Objective OI-Indicators

	Obj. Index	DSR > 0.4	RDSR > 0.4	Paid Late	> 2 Loans
	(1)	(2)	(3)	(4)	(5)
Very Negative	−0.098	−0.024	−0.032	−0.027	−0.031
	(0.154)	(0.055)	(0.085)	(0.040)	(0.067)
Negative	−0.016	−0.064	0.076	0.045	−0.069
	(0.141)	(0.051)	(0.064)	(0.035)	(0.072)
Neutral	0.094	0.002	0.041	0.083	−0.023
	(0.197)	(0.070)	(0.067)	(0.060)	(0.078)
Positive	0.352**	0.093	0.212***	0.132***	−0.023
	(0.153)	(0.055)	(0.073)	(0.042)	(0.064)
Constant	−1.394*	0.082	−0.634**	0.076	−0.299
	(0.676)	(0.340)	(0.292)	(0.236)	(0.308)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	575	575	575	572	575
Adj. R-squared	0.094	0.040	0.141	0.046	0.046

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.5: Subsample Financial Decision Makers: Subjective OI-Indicators

	Subj. Index	Debt Position	Diff. Pay off Debt	Sacrifice Index
	(1)	(2)	(3)	(4)
Very Negative	0.141	−0.041	0.047	0.204*
	(0.122)	(0.134)	(0.040)	(0.116)
Negative	0.108	−0.042	0.021	0.245
	(0.120)	(0.116)	(0.027)	(0.208)
Neutral	−0.030	−0.053	0.013	−0.074
	(0.115)	(0.114)	(0.018)	(0.135)
Positive	0.252**	0.148**	0.040	0.278*
	(0.100)	(0.069)	(0.026)	(0.156)
Constant	−0.181	−1.442**	0.194	0.848
	(0.710)	(0.563)	(0.179)	(0.787)
Controls	Yes	Yes	Yes	Yes
Observations	575	575	573	575
Adj. R-squared	0.140	0.108	0.065	0.132

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.6: Interaction of Over-Indebtedness Indices with Conscientiousness

	Obj. Index	**Subj. Debt Index**
	(1)	(2)
Very Negative	−0.409	1.102
	(0.747)	(0.867)
Negative	−0.767	0.834
	(0.498)	(0.668)
Neutral	−0.184	0.169
	(0.801)	(0.596)
Positive	−0.071	0.909
	(0.773)	(0.592)
Conscientiousness	−0.105	0.077
	(0.069)	(0.085)
Very neg. x Conscient.	0.068	−0.155
	(0.127)	(0.140)
Negative x Conscient.	0.144*	−0.119
	(0.076)	(0.107)
Neutral x Conscient.	0.056	−0.021
	(0.127)	(0.103)
Positive x Conscient.	0.071	−0.113
	(0.122)	(0.106)
Constant	−0.859	−0.942
	(0.777)	(0.769)
Controls	Yes	Yes
Observations	676	676
Adj. R-squared	0.095	0.130

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.7: Objective Over-Indebtedness, Quantitative Inc. Forecast Dummy

	Obj. Index		DSR > 0.4		RDSR > 0.4		Paid Late/Default		> 2 Loans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy	0.269**	0.245**	0.131***	0.095**	0.163***	0.137**	0.058*	0.077**	−0.033	−0.022
	(0.097)	(0.101)	(0.036)	(0.038)	(0.049)	(0.049)	(0.031)	(0.034)	(0.035)	(0.040)
Farm. Shocks		−0.000		−0.000		−0.000		−0.000		0.000
		(0.002)		(0.000)		(0.001)		(0.001)		(0.001)
Env. Shocks		0.005***		−0.000		0.002***		0.002*		0.002***
		(0.001)		(0.001)		(0.000)		(0.001)		(0.001)
Econ. Shocks		0.003***		0.000		0.002***		0.001*		0.000
		(0.001)		(0.000)		(0.001)		(0.001)		(0.001)
Crime Shocks		−0.014		−0.003		−0.012***		−0.001		−0.001
		(0.009)		(0.002)		(0.003)		(0.004)		(0.004)
Other Shocks		−0.000		−0.000		−0.000		0.000*		−0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Constant	−0.061	−1.274**	0.150***	0.133	0.358***	−0.518*	0.141***	0.074	0.237***	−0.314
	(0.091)	(0.546)	(0.031)	(0.285)	(0.042)	(0.294)	(0.015)	(0.226)	(0.044)	(0.265)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	686	676	686	676	686	676	683	673	686	676
Adj. R-squ.	0.012	0.099	0.020	0.048	0.019	0.121	0.003	0.037	-0.000	0.055

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.8: Subjective Over-Indebtedness, Quantitative Inc. Forecast Dummy

	Subj. Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forecast Dummy	0.063	0.172*	0.105	0.165*	−0.005	0.019	0.054	0.146
	(0.097)	(0.093)	(0.094)	(0.086)	(0.020)	(0.024)	(0.079)	(0.087)
Farming Shocks		−0.001		0.001		−0.000**		−0.003
		(0.001)		(0.001)		(0.000)		(0.002)
Environmental Shocks		0.007***		0.003***		0.002***		0.004*
		(0.001)		(0.001)		(0.001)		(0.002)
Economic Shocks		0.000		0.003**		−0.001		−0.000
		(0.001)		(0.001)		(0.000)		(0.002)
Crime Shocks		0.000		−0.006		0.003		−0.006
		(0.014)		(0.008)		(0.003)		(0.015)
Other Shocks		0.002***		0.000		0.001***		0.002***
		(0.001)		(0.000)		(0.000)		(0.000)
Constant	−0.037	−0.430	−0.044	−1.447***	0.066***	0.152	−0.100*	0.377
	(0.040)	(0.566)	(0.045)	(0.504)	(0.011)	(0.147)	(0.050)	(0.584)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	686	676	686	676	684	674	686	676
Adj. R-squared	-0.001	0.133	0.001	0.099	-0.001	0.073	-0.001	0.117

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.9: Certainty Measure - Objective Over-Indebtedness - Quantitative Inc. Forecast Dummy

	Obj. Index	DSR > 0.4	RDSR > 0.4	Paid Late	> 2 Loans
	(1)	(2)	(3)	(4)	(5)
Forecast Dummy	0.242**	0.096**	0.130**	0.079**	−0.023
	(0.103)	(0.040)	(0.050)	(0.035)	(0.041)
Certainty	0.127*	0.053**	0.043	−0.008	0.062**
	(0.061)	(0.023)	(0.027)	(0.024)	(0.022)
Constant	−1.406**	0.080	−0.587*	0.160	−0.443
	(0.526)	(0.286)	(0.299)	(0.247)	(0.262)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	664	664	664	661	664
Adj. R-squared	0.102	0.056	0.121	0.035	0.063

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.1.10: Certainty Measure - Subjective Over-Indebtedness - Quantitative Inc. Forecast Dummy

	Subj. Index	Debt Position	Diff. Pay off Debt	Sacrifice Index
	(1)	(2)	(3)	(4)
Forecast Dummy	0.156	0.160*	0.014	0.133
	(0.094)	(0.091)	(0.022)	(0.091)
Certainty	0.064	0.090	0.005	0.023
	(0.089)	(0.066)	(0.021)	(0.107)
Constant	−0.609	−1.761***	0.154	0.331
	(0.630)	(0.571)	(0.153)	(0.726)
Controls	Yes	Yes	Yes	Yes
Observations	664	664	662	664
Adj. R-squared	0.133	0.103	0.072	0.112

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

D.2 The Qualitative Forecast Error

Deriving the Qualitative Forecast Error

We develop a measure of expectation accuracy closely following Souleles (2004) and Hyytinen and Putkuri (2018), which enables us to replicate the latter authors' results. We make use of the available panel data and combine categorical answers to the question, "How do you think your average monthly income will develop in the next twelve months?" ($E_{t-1}(Inc_{i,t})$) asked in 2016 (one year prior to our survey) with responses to the question "Do you think your household is better off than last year" asked in 2017 ($A(Inc_{i,t})$).[1] We call the difference between these two questions qualitative forecast error:

$$Qualitative\ Forecast\ Error = A(Inc_{i,t}) - E_{t-1}(Inc_{i,t}) \tag{D.2.1}$$

A positive qualitative forecast error occurs if the expected household situation is better than the realized one and a negative if the opposite is true. We form five categories ranging from a very negative to a very positive qualitative forecast error, which enter the regression analysis as dummy variables. The category with households making no forecast error serves as omitted group.

As the qualitative forecast error is derived at the household level, the respondent may not be the same for all three data points. Therefore, we re-run the analysis for a sub-sample with only identical respondents, which does not change the results. We assume that the household's qualitative assessment regarding its own development stays similar for a time period of two years and, thus, is able to explain indebtedness in 2017. There are two reasons encouraging this view: We are able to control for a rich set of socio-economic variables that capture household formation and, as incomes are rather stationary, expectations may also change slowly.

Results for the Qualitative Forecast Error

The regressions we run for the qualitative forecast error take the same form as the ones for the quantitative income forecast (standard errors are clustered at the district level):

$$Over-Indebtedness\ Index_i = \beta_0 + \beta_1 Qual.FE_i + X_i^{'}\beta_2 + \epsilon_i \tag{D.2.2}$$

Results for the objective and subjective OI-Indices are presented in Tables D.2.1 and D.2.2. With regards to the relationship between the objective OI-Index and the qualitative forecast error, we find that over-indebtedness increases by 0.42 points if respondents exhibit a very positive forecast error. The results are driven by two components: the remaining debt to

[1] Answer options range on a scale from 1-5. For the question asked in 2016, one means "increase a lot" and five "decrease a lot." The question asked in 2017 ranges from one being "much better off" to five "much worse off."

service ratio (columns (5) and (6), Table D.2.1) and the probability of whether people paid late or defaulted (columns (7) and (8)). The results are similar to those of the quantitative income forecast. We again find that very positive forecasts are related to a higher probability of being objectively over-indebted. Point estimates are slightly higher for results from the qualitative forecast error. Regarding the impact of losses from shocks as well as additional control variables, results are similar to those of the quantitative income forecast. Overall, results from the qualitative forecast error confirm the findings from the quantitative income forecast: positive future income expectations are related to increasing objective over-indebtedness.

Table D.2.1: Qualitative Forecast Error - Main Results Objective OI-Indicators

	Obj. Index		DSR > 0.4		RDSR > 0.4		Paid Late/Default		> 2 Loans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Very Negative	0.130	0.179	−0.089	−0.073	0.118	0.148	0.106	0.109	0.024	0.034
	(0.222)	(0.236)	(0.061)	(0.067)	(0.129)	(0.142)	(0.106)	(0.101)	(0.067)	(0.065)
Negative	−0.158**	−0.055	−0.046	−0.030	−0.033	0.006	−0.026	−0.003	−0.076	−0.033
	(0.063)	(0.069)	(0.032)	(0.035)	(0.040)	(0.044)	(0.028)	(0.029)	(0.047)	(0.048)
Positive	0.165**	0.069	0.007	−0.009	0.087*	0.044	0.035	0.014	0.069	0.034
	(0.064)	(0.070)	(0.031)	(0.034)	(0.045)	(0.040)	(0.036)	(0.035)	(0.041)	(0.039)
Very Positive	0.443**	0.410**	0.070	0.052	0.194***	0.182***	0.151*	0.149**	0.100	0.093
	(0.170)	(0.144)	(0.073)	(0.068)	(0.058)	(0.050)	(0.073)	(0.067)	(0.063)	(0.057)
Farm.Shocks		0.000		0.000		−0.000		0.000		0.000
		(0.001)		(0.000)		(0.000)		(0.000)		(0.000)
Env. Shocks		0.003**		−0.000		0.002***		0.001		0.001**
		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)
Econ. Shocks		0.003***		0.001**		0.001***		0.001**		0.000
		(0.001)		(0.000)		(0.000)		(0.000)		(0.000)
Crime Shocks		−0.012***		−0.003***		−0.006**		−0.002		−0.003
		(0.004)		(0.001)		(0.002)		(0.002)		(0.002)
Other Shocks		−0.000		−0.000**		−0.000		0.000		−0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Constant	−0.059	−1.264**	0.184***	0.190	0.359***	−0.508*	0.132***	0.059	0.214***	−0.355
	(0.082)	(0.584)	(0.032)	(0.320)	(0.032)	(0.290)	(0.020)	(0.229)	(0.038)	(0.275)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	688	676	688	676	688	676	685	673	688	676
Adj. R-squ.	0.022	0.120	0.002	0.044	0.014	0.124	0.013	0.050	0.011	0.063

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, positive*, and *very positive* represent the forecast groups. Households with no forecast error serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

We also find a strongly significant relationship between positive qualitative forecast errors and subjective over-indebtedness. This relationship is much stronger than for the quantitative income forecast. Again, we only find a robust relationship for households in the group with the largest positive forecasts. The subjective OI-Index increases by 0.42 points for respondents who exhibit very positive forecast errors (columns (1) and (2), Table D.2.2). Mainly, this is due to the positive relationship between the forecast error and the "debt position" component of the index and the sacrifice index component.

Table D.2.2: Qualitative Forecast Error - Main Results Subjective OI-Indicators

	Subj. Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Very Negative	0.218	0.140	0.064	0.055	0.052	0.027	0.243	0.167
	(0.258)	(0.245)	(0.230)	(0.261)	(0.068)	(0.060)	(0.214)	(0.198)
Negative	−0.025	0.030	−0.091	−0.011	0.030	0.028	−0.096	−0.046
	(0.127)	(0.103)	(0.072)	(0.061)	(0.035)	(0.031)	(0.153)	(0.134)
Positive	0.208**	0.105	0.139*	0.065	0.021	0.011	0.265*	0.134
	(0.077)	(0.083)	(0.072)	(0.069)	(0.016)	(0.019)	(0.150)	(0.133)
Very Positive	0.476**	0.455**	0.351*	0.361**	0.091	0.086	0.352*	0.308*
	(0.208)	(0.186)	(0.177)	(0.155)	(0.053)	(0.053)	(0.187)	(0.160)
Farming Shocks		0.000		0.000		0.000		−0.001
		(0.001)		(0.001)		(0.000)		(0.001)
Environ. Shocks		0.002		0.001		0.001		0.001
		(0.002)		(0.001)		(0.001)		(0.002)
Economic Shocks		0.000		0.002**		−0.000*		0.001
		(0.001)		(0.001)		(0.000)		(0.001)
Crime Shocks		−0.003		−0.000		−0.000		−0.007
		(0.007)		(0.007)		(0.002)		(0.006)
Other Shocks		0.001***		0.000		0.000***		0.001**
		(0.000)		(0.000)		(0.000)		(0.000)
Constant	−0.122**	−0.499	−0.074	−1.459**	0.043***	0.122	−0.176**	0.357
	(0.057)	(0.664)	(0.050)	(0.530)	(0.014)	(0.175)	(0.072)	(0.626)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	688	676	688	676	686	674	688	676
Adj. R-squared	0.019	0.136	0.015	0.102	0.006	0.073	0.012	0.115

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, positive*, and *very positive* represent the forecast groups. Households with no forecast error serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Households with a very positive error tend to state more frequently that they "have too much debt right now" (columns (3) and (4)) and that they make an increasing number of everyday sacrifices to repay their loans (column (7) and (8)). We conclude that the nature of the qualitative forecast error being more "subjectively" elicited than the calculated quantitative income forecast *per se*, might be reflected in more pronounced results regarding subjectively "felt" debt. This is also in line with our analysis from the quantitative income forecast that subjective over-indebtedness may rather be a concept of perceived financial distress affected by not only the household's true debt situation but also by respondent characteristics.

Furthermore, we again add income certainty to the regression. Results are presented in Tables D.2.3 and D.2.4. There is no relationship between future income certainty on objective and subjective over-indebtedness. For the subjective OI-Indicators, results are in line with those from the quantitative income forecast. However, they differ for objective over-indebtedness.

Table D.2.3: Objective Over-Indebtedness - Income Certainty

	Obj. Index	DSR > 0.4	RDSR > 0.4	Paid Late	> 2 Loans
	(1)	(2)	(3)	(4)	(5)
Very Negative	0.180	−0.075	0.151	0.110	0.034
	(0.242)	(0.067)	(0.145)	(0.102)	(0.066)
Negative	−0.056	−0.030	0.007	−0.004	−0.034
	(0.068)	(0.035)	(0.044)	(0.029)	(0.048)
Positive	0.070	−0.010	0.045	0.015	0.034
	(0.069)	(0.034)	(0.040)	(0.035)	(0.039)
Very Positive	0.465**	0.093	0.187***	0.153*	0.104*
	(0.164)	(0.078)	(0.059)	(0.074)	(0.058)
Certainty	0.046	0.030	0.008	0.004	0.011
	(0.049)	(0.020)	(0.017)	(0.019)	(0.024)
Constant	−1.481**	−0.001	−0.640**	0.066	−0.297
	(0.551)	(0.295)	(0.280)	(0.262)	(0.261)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	663	663	663	660	663
Adj. R-squared	0.118	0.050	0.122	0.046	0.058

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered S.E. in parentheses. The variables *very negative, negative, positive*, and *very positive* represent the forecast groups. Households with no forecast error serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

While we find that higher certainty is related to higher objective over-indebtedness with respect to the quantitative income forecast, we do not find that relationship with the qualitative error. This may be due to the more subjective nature of the qualitative forecast error.

Table D.2.4: Subjective Over-Indebtedness - Income Certainty

	Subj. Index	Debt Position	Diff. Pay off Debt	Sacrifice Index
	(1)	(2)	(3)	(4)
Very Negative	0.150	0.063	0.026	0.186
	(0.242)	(0.258)	(0.060)	(0.192)
Negative	0.028	−0.012	0.028	−0.048
	(0.104)	(0.061)	(0.031)	(0.136)
Positive	0.109	0.068	0.011	0.141
	(0.085)	(0.071)	(0.019)	(0.135)
Very Positive	0.578**	0.429**	0.116*	0.400**
	(0.211)	(0.191)	(0.064)	(0.169)
Certainty	−0.035	−0.033	0.010	−0.103
	(0.058)	(0.048)	(0.012)	(0.072)
Constant	−0.356	−1.374**	0.128	0.605
	(0.667)	(0.563)	(0.181)	(0.629)
Controls	Yes	Yes	Yes	Yes
Observations	663	663	661	663
Adj. R-squared	0.143	0.104	0.076	0.121

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered S.E. in parentheses. The variables *very negative, negative, positive*, and *very positive* represent the forecast groups. Households with no forecast error serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Overall, results from the qualitative forecast error confirm the main findings from the quantitative income forecast: very positive forecasts are related to a higher level of over-indebtedness. There is no such relationship for negative forecasts and over-indebtedness. The results also support the analysis from the quantitative income forecast that subjective and objective over-indebtedness indicators measure different dimensions of indebtedness. Finally, our results from the qualitative forecast error are in line with those of Hyytinen and Putkuri (2018). They report that households with a very positive forecast error are more likely to be over-indebted and that such a pattern cannot be found for households with negative forecast errors. Our results show the same relationship.

D.3 Experiment Appendix

Table D.3.1: Descriptive Statistics by Participation in Game

	Full Sample	Participating	Non-Participating	Difference
Sex	1.66	1.63	1.76	0.12***
Age	57.01	56.35	59.78	3.43***
Relation to HH Head	1.67	1.66	1.71	0.05
Marital Status	2.15	2.14	2.22	0.09
Main Occupation	4.97	4.66	6.29	1.64*
Years of Schooling	5.74	5.83	5.33	−0.51*
Children (0-6 years)	0.32	0.32	0.33	0.01
Children (7-10 years)	0.24	0.23	0.25	0.02
Numeracy	2.05	2.13	1.69	−0.45***
Health Status	1.40	1.38	1.46	0.08
BMI	23.64	23.70	23.41	−0.28
Fin. Decision Maker	1.57	1.56	1.60	0.03
Self Control	21.26	21.02	22.26	1.24
Risk Taking	3.95	3.99	3.78	−0.21
Fin. Risk Taking	3.94	4.04	3.57	−0.47**
FL-Score	5.50	5.63	4.95	−0.68***
Monthly Inc. 2017	19197.02	19313.71	18704.57	−609.14
Obj. OI-Index	0.00	0.00	−0.00	−0.00
Subj. OI-Index	−0.00	−0.01	0.03	0.04
Morning	0.53	0.53	0.53	0.00
Midday	0.24	0.26	0.17	−0.09***
Observations	748	604	144	748

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels.

Table D.3.2: Linear Probability Model Participation in Game

	Participation
Sex	−0.077**
	(0.036)
Age	−0.003**
	(0.002)
Fin. Risk Taking	0.023**
	(0.010)
FL-Score	0.020**
	(0.010)
Morning	0.083**
	(0.040)
Midday	0.144***
	(0.043)
Observations	717

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Only significant variables reported, remaining variables are the same as in Table D.3.1.

Table D.3.3: Descriptive Statistics for Excluded Sample

	Full Sample	In	Out	Difference
Sex	1.65	1.64	1.67	−0.03
Age	56.40	56.16	57.75	−1.59
Relation to HH Head	1.68	1.70	1.56	0.14
Marital Status	2.14	2.13	2.24	−0.11
Main Occupation	4.68	4.79	4.08	0.71
Years of Schooling	5.87	5.92	5.60	0.32
Children (0-6 years)	0.31	0.33	0.25	0.08
Children (7-10 years)	0.24	0.26	0.13	0.13***
Numeracy	2.13	2.14	2.04	0.11
Health Status	1.38	1.38	1.38	0.00
BMI	23.69	23.58	24.27	−0.68
Fin. Decision Maker	1.56	1.57	1.52	0.05
Self Control	21.05	20.94	21.62	−0.67
Risk Taking	3.98	4.02	3.74	0.28
Fin. Risk Taking	4.03	4.06	3.90	0.15
FL-Score	5.62	5.66	5.40	0.26
Monthly Inc. 2017	18523.65	18653.06	17798.04	855.02
Obj. OI-Index	0.01	0.01	−0.02	0.03
Subj. OI-Index	−0.03	−0.04	0.05	−0.09
Read Alone	1.45	1.44	1.49	−0.04
Difficulties	1.15	1.14	1.21	−0.08
Observations	555	471	84	555

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels.

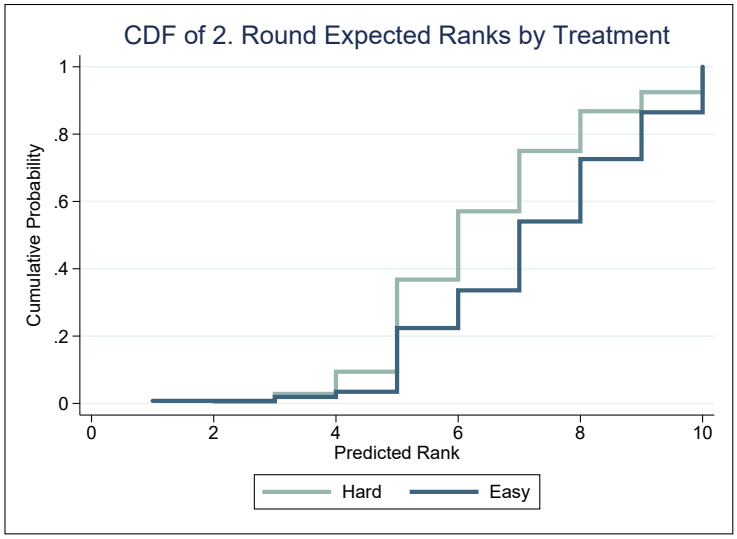


Figure D.3.1: CDF for the Expected Rank by Treatment, After the Main Quiz

The Rationals

As mentioned above, so far we have excluded experiment participants who want to buy more than they expect to earn. We refer to these persons as "rationals." In this section, we discuss whether these participants are actually rational or had difficulties in understanding the experiment and how including these observations change our results. Comparing our main sample against all rationals does not yield results that differ substantially from those presented in Table D.3.3. However, if we divide the rationals into those participants who want to buy more than expected earnings could pay for but less than eight goods and those who want to buy exactly eight goods (which would be the "truly" rational decision), we find interesting differences. The former group has significantly lower education, numeracy, and financial literacy than the main sample (see Table D.3.4). We see this as evidence that they may have had difficulties understanding the game (from here on, we refer to these individuals as non-rationals). It does not seem to be the case, however, that these are persons who generally have problems controlling their own spending behavior (also outside the lab) because their debt to service ratio is significantly smaller compared to the main sample.

The remaining rationals, however, not only have significantly higher numeracy and financial literacy, but also have a better understanding of the game as perceived by the interviewers (see Table D.3.5) (for non-rationals the difference is in the opposite direction, but not significant). Thus, these participants might have taken advantage of the set-up and reasoned that it is optimal for them to buy as many goods as possible because of the large discount.

Including these two groups into the analysis, the results change as anticipated: the effect of expected rank on goods turns insignificant and negligible (see Table D.3.6). All other effects are almost unchanged.

Table D.3.4: Descriptive Statistics for Non-Rationals (only significant effects reported)

	Full Sample	Others	Non-Rationals	Difference
Years of Schooling	5.84	5.91	5.00	0.91***
Children (7-10 years)	0.24	0.26	0.12	0.14**
Numeracy	2.10	2.13	1.76	0.36*
FL-Score	5.60	5.64	5.10	0.54*
Observations	532	490	42	532

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels.

Table D.3.5: Descriptive Statistics for Rationals (only significant effects reported)

	Full Sample	Others	Rationals	Difference
Main Occupation	4.70	4.76	3.48	1.28*
Numeracy	2.16	2.13	2.78	−0.66*
FL-Score	5.66	5.64	6.22	−0.58*
Difficulties in Game	1.15	1.16	1.00	0.16***
Observations	513	490	23	513

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels.

Table D.3.6: Consumption Decision including Rationals

	Exp. Rank	No. Goods		
	(1)	(2)	(3)	(4)
Treatment	0.373**	−0.234		−0.254
	(0.168)	(0.199)		(0.199)
Exp. Rank			0.048	0.054
			(0.052)	(0.052)
Controls	Yes	Yes	Yes	Yes
Observations	511	511	511	511

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Standard errors in parentheses. Treatment: 0=Hard Quiz, 1=Easy Quiz; A higher expected rank corresponds to a higher expected performance. Controls: Health Status, Monthly HH income and Objective OI-Index.

D.4 Online Appendix

Additional Regression Tables

Table D.4.1: Additional Regression on Big 5 Measures - Objective Over-Indebtedness

	Obj. Index		DSR > 0.4		RDSR > 0.4		Paid Late/Default		> 2 Loans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Very Negative	−0.125	−0.032	−0.097*	−0.026	−0.073	0.006	0.017	−0.021	0.001	0.008
	(0.151)	(0.137)	(0.047)	(0.050)	(0.081)	(0.076)	(0.033)	(0.036)	(0.059)	(0.059)
Negative	0.050	0.056	−0.067	−0.052	0.075	0.097*	0.081**	0.062**	−0.029	−0.035
	(0.134)	(0.133)	(0.045)	(0.050)	(0.058)	(0.056)	(0.032)	(0.029)	(0.057)	(0.061)
Neutral	0.153	0.111	0.025	−0.001	0.079	0.059	0.074	0.087*	−0.002	−0.019
	(0.153)	(0.160)	(0.050)	(0.058)	(0.058)	(0.060)	(0.045)	(0.050)	(0.061)	(0.063)
Positive	0.289**	0.311**	0.098**	0.084*	0.187**	0.206***	0.109***	0.128***	−0.054	−0.050
	(0.134)	(0.135)	(0.042)	(0.046)	(0.072)	(0.072)	(0.038)	(0.040)	(0.055)	(0.060)
Openness		0.100***		0.028***		0.040**		0.027**		0.022
		(0.030)		(0.008)		(0.016)		(0.012)		(0.016)
Conscient.		−0.083**		−0.016		−0.036**		−0.025		−0.020
		(0.031)		(0.014)		(0.014)		(0.016)		(0.013)
Extraversion		−0.003		0.013		−0.013		−0.018		0.014
		(0.038)		(0.013)		(0.021)		(0.015)		(0.015)
Agreeabl.		0.039		0.007		−0.008		0.009		0.034*
		(0.049)		(0.019)		(0.019)		(0.019)		(0.019)
Neuroticism		0.033		0.001		0.008		0.002		0.029*
		(0.034)		(0.010)		(0.018)		(0.017)		(0.015)
Constant	−0.073	−1.493*	0.189***	0.053	0.343***	−0.464	0.099***	0.073	0.245***	−0.539*
	(0.144)	(0.783)	(0.048)	(0.367)	(0.072)	(0.360)	(0.019)	(0.264)	(0.063)	(0.305)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	688	676	688	676	688	676	685	673	688	676
Adj. R-squ.	0.014	0.108	0.025	0.047	0.025	0.129	0.007	0.046	-0.003	0.061

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.4.2: Additional Regression on Big 5 Measures - Subjective Over-Indebtedness

	Subj. Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Very Negative	0.182	0.213*	0.040	0.035	0.065**	0.056	0.118	0.252**
	(0.112)	(0.115)	(0.114)	(0.103)	(0.029)	(0.039)	(0.106)	(0.102)
Negative	0.157	0.136	0.096	0.032	0.037	0.034	0.108	0.155
	(0.135)	(0.113)	(0.111)	(0.109)	(0.025)	(0.026)	(0.174)	(0.157)
Neutral	−0.007	0.030	−0.021	−0.003	0.022	0.030	−0.098	−0.061
	(0.104)	(0.089)	(0.096)	(0.090)	(0.021)	(0.020)	(0.128)	(0.100)
Positive	0.144	0.239**	0.113	0.170**	0.024	0.041*	0.113	0.206*
	(0.086)	(0.091)	(0.071)	(0.077)	(0.021)	(0.023)	(0.120)	(0.113)
Openness		0.094**		0.058*		0.012		0.113**
		(0.036)		(0.033)		(0.009)		(0.049)
Conscientiousness		−0.007		0.005		−0.017		0.054
		(0.054)		(0.042)		(0.014)		(0.056)
Extraversion		−0.042		−0.055		0.007		−0.072
		(0.042)		(0.037)		(0.012)		(0.042)
Agreeableness		−0.021		−0.026		−0.001		−0.019
		(0.042)		(0.037)		(0.011)		(0.050)
Neuroticism		0.058*		0.031		−0.002		0.123**
		(0.031)		(0.029)		(0.009)		(0.044)
Constant	−0.115	−0.577	−0.064	−1.401**	0.035**	0.183	−0.131	−0.209
	(0.082)	(0.706)	(0.081)	(0.646)	(0.016)	(0.154)	(0.111)	(0.812)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	688	676	688	676	686	674	688	676
Adj. R-squared	0.001	0.143	-0.002	0.098	0.002	0.072	-0.001	0.141

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses. The variables *very negative, negative, neutral*, and *positive* represent the income forecast groups. Households with a mildly negative income forecast serve as the reference group. Additional controls: age, age squared, children (0-6), children (7-10), children (11-16), financial literacy score, loss from crime shocks, loss from economic shocks, loss from environmental shocks, loss from other shocks, main income farming, main income employed, main income self-employed, main income remittances, monthly household income 2017, no. of elders in hh, no. of working members in hh, risk preference, self-control, social status, total hh education.

Table D.4.3: Full Regression Output for Main Regression - Objective OI

	Obj. Index		DSR > 0.4		RDSR > 0.4		Paid Late/Default		> 2 Loans	
Very Negative	−0.125	−0.017	−0.097*	−0.022	−0.073	0.011	0.017	−0.015	0.001	0.010
	(0.151)	(0.143)	(0.047)	(0.050)	(0.081)	(0.079)	(0.033)	(0.036)	(0.059)	(0.060)
Negative	0.050	0.058	−0.067	−0.054	0.075	0.100*	0.081**	0.066**	−0.029	−0.037
	(0.134)	(0.132)	(0.045)	(0.048)	(0.058)	(0.057)	(0.032)	(0.029)	(0.057)	(0.058)
Neutral	0.153	0.135	0.025	0.002	0.079	0.067	0.074	0.095*	−0.002	−0.010
	(0.153)	(0.168)	(0.050)	(0.060)	(0.058)	(0.064)	(0.045)	(0.051)	(0.061)	(0.063)
Positive	0.289**	0.333**	0.098**	0.087*	0.187**	0.210***	0.109***	0.133***	−0.054	−0.037
	(0.134)	(0.136)	(0.042)	(0.047)	(0.072)	(0.069)	(0.038)	(0.041)	(0.055)	(0.060)
Monthly Inc. 2017		−0.000		−0.000*		−0.000***		0.000		0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Age		0.061***		0.007		0.031***		0.015*		0.019***
		(0.017)		(0.008)		(0.009)		(0.008)		(0.007)
Age Squared		−0.001***		−0.000		−0.000***		−0.000**		−0.000***
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
FL-Score		0.021		0.008		0.018***		−0.010		0.012
		(0.018)		(0.007)		(0.006)		(0.006)		(0.007)
Risk Preference		0.054***		0.013*		0.026***		0.012		0.013
		(0.018)		(0.007)		(0.008)		(0.008)		(0.008)
Self-Control		0.001		−0.002		−0.001		0.003		0.001
		(0.006)		(0.002)		(0.003)		(0.002)		(0.002)
Main Inc. Farming		−0.122		−0.066		−0.006		−0.090		0.032
		(0.155)		(0.059)		(0.091)		(0.057)		(0.044)
Main Inc. Employed		−0.194		−0.106*		−0.032		−0.022		−0.063
		(0.166)		(0.059)		(0.076)		(0.057)		(0.055)
Main Inc. Self-Emp.		−0.163		−0.087		−0.025		−0.025		−0.053
		(0.212)		(0.089)		(0.099)		(0.068)		(0.061)
Main Inc. Remitt.		−0.151		−0.068		−0.016		−0.070		−0.015
		(0.144)		(0.060)		(0.057)		(0.058)		(0.037)
Children (0-6 yrs)		−0.085*		−0.012		−0.057**		0.007		−0.045**
		(0.047)		(0.017)		(0.026)		(0.027)		(0.020)
Children (7-10 yrs)		0.092		0.012		0.079**		0.008		0.019
		(0.082)		(0.048)		(0.033)		(0.022)		(0.036)
Children (11-16 yrs)		0.030		−0.017		0.017		0.025		0.009
		(0.040)		(0.020)		(0.021)		(0.020)		(0.019)
No. of Elders		0.036		0.003		0.036*		0.034*		−0.032
		(0.040)		(0.024)		(0.018)		(0.020)		(0.023)
No. of Working Mem.		0.072*		0.022		0.008		0.002		0.051**
		(0.042)		(0.015)		(0.019)		(0.019)		(0.021)
Total HH Education		−0.001		−0.000		0.002		−0.000		−0.003
		(0.005)		(0.002)		(0.003)		(0.002)		(0.002)
Farming Shocks		−0.000		−0.000		0.000		−0.000		0.000
		(0.002)		(0.000)		(0.001)		(0.001)		(0.001)
Environ. Shocks		0.005***		−0.000		0.002***		0.002**		0.002***
		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)
Economic Shocks		0.003***		0.000		0.002***		0.001*		0.000
		(0.001)		(0.000)		(0.001)		(0.001)		(0.001)
Crime Shocks		−0.016*		−0.004*		−0.013***		−0.002		−0.001
		(0.009)		(0.002)		(0.003)		(0.004)		(0.004)
Other Shocks		−0.000		−0.000		−0.000		0.000**		−0.000
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Social Status		−0.140*		−0.021		−0.028		−0.056***		−0.051
		(0.071)		(0.023)		(0.033)		(0.019)		(0.032)
Constant	−0.073	−1.425**	0.189***	0.119	0.343***	−0.617**	0.099***	−0.016	0.245***	−0.291
	(0.144)	(0.576)	(0.048)	(0.296)	(0.072)	(0.286)	(0.019)	(0.243)	(0.063)	(0.280)
Observations	688	676	688	676	688	676	685	673	688	676
Adj. R-squared	0.014	0.099	0.025	0.046	0.025	0.125	0.007	0.044	-0.003	0.053

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table D.4.4: Full Regression Output for Main Regression - Subjective OI

	Subj. Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
Very Negative	0.182	0.215*	0.040	0.036	0.065**	0.058	0.118	0.245**
	(0.112)	(0.122)	(0.114)	(0.110)	(0.029)	(0.039)	(0.106)	(0.103)
Negative	0.157	0.150	0.096	0.046	0.037	0.033	0.108	0.178
	(0.135)	(0.110)	(0.111)	(0.109)	(0.025)	(0.026)	(0.174)	(0.154)
Neutral	−0.007	0.048	−0.021	0.008	0.022	0.031	−0.098	−0.035
	(0.104)	(0.092)	(0.096)	(0.094)	(0.021)	(0.019)	(0.128)	(0.095)
Positive	0.144	0.258**	0.113	0.181**	0.024	0.041*	0.113	0.245*
	(0.086)	(0.101)	(0.071)	(0.084)	(0.021)	(0.023)	(0.120)	(0.122)
Monthly Inc. 2017		−0.000		−0.000		−0.000		−0.000
		(0.000)		(0.000)		(0.000)		(0.000)
Age		0.061***		0.063***		0.007*		0.042**
		(0.014)		(0.015)		(0.004)		(0.018)
Age Squared		−0.001***		−0.001***		−0.000**		−0.000**
		(0.000)		(0.000)		(0.000)		(0.000)
FL-Score		−0.026**		0.007		−0.007**		−0.047**
		(0.012)		(0.010)		(0.003)		(0.018)
Risk Preference		0.044**		0.057***		0.003		0.023
		(0.017)		(0.018)		(0.005)		(0.019)
Self-Control		0.009**		0.005		0.001		0.015***
		(0.004)		(0.004)		(0.001)		(0.005)
Main Inc. Farming		−0.192**		−0.159		0.007		−0.323**
		(0.078)		(0.100)		(0.032)		(0.140)
Main Inc. Employed		0.042		0.017		0.047		−0.138
		(0.121)		(0.114)		(0.037)		(0.176)
Main Inc. Self-Emp.		−0.019		−0.019		0.031		−0.178
		(0.139)		(0.108)		(0.046)		(0.164)
Main Inc. Remitt.		−0.159		−0.251**		0.020		−0.176
		(0.102)		(0.090)		(0.036)		(0.165)
Children (0-6 yrs)		−0.091		−0.101**		−0.012		−0.046
		(0.062)		(0.048)		(0.016)		(0.063)
Children (7-10 yrs)		−0.084		0.039		−0.026		−0.162
		(0.075)		(0.071)		(0.019)		(0.094)
Children (11-16 yrs)		0.007		−0.002		−0.022		0.123*
		(0.063)		(0.037)		(0.022)		(0.066)
No. of Elders		0.026		0.043		0.012		−0.045
		(0.036)		(0.042)		(0.011)		(0.056)
No. of Working Mem.		0.121***		0.123***		−0.005		0.182***
		(0.042)		(0.033)		(0.014)		(0.045)
Total HH Education		−0.009**		−0.008**		0.001		−0.019***
		(0.004)		(0.003)		(0.001)		(0.005)
Farming Shocks		−0.001		0.002		−0.000*		−0.002
		(0.001)		(0.001)		(0.000)		(0.002)
Environmental Shocks		0.007***		0.003***		0.002**		0.003
		(0.001)		(0.001)		(0.001)		(0.002)
Economic Shocks		0.001		0.003**		−0.000		−0.000
		(0.001)		(0.001)		(0.000)		(0.002)
Crime Shocks		0.000		−0.006		0.003		−0.005
		(0.014)		(0.007)		(0.003)		(0.014)
Other Shocks		0.002***		0.000		0.001***		0.002***
		(0.001)		(0.000)		(0.000)		(0.000)
Social Status		−0.353***		−0.184***		−0.069***		−0.371***
		(0.079)		(0.045)		(0.023)		(0.092)
Constant	−0.115	−0.482	−0.064	−1.480***	0.035**	0.140	−0.131	0.344
	(0.082)	(0.593)	(0.081)	(0.514)	(0.016)	(0.155)	(0.111)	(0.591)
Observations	688	676	688	676	686	674	688	676
Adj. R-squared	0.001	0.133	-0.002	0.094	0.002	0.073	-0.001	0.119

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table D.4.5: Additional Regression on Predictors for Income Forecast Groups

	Very Negative		Negative		Mildly Negative		Neutral		Positive	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Monthly Inc. 2017	0.000***	0.000***	0.000	0.000	−0.000**	−0.000**	−0.000***	−0.000***	−0.000***	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	0.016**	0.018***	0.003	0.000	0.004	0.006	−0.008	−0.006	−0.015*	−0.019**
	(0.006)	(0.006)	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)
Age Squared	−0.000**	−0.000**	−0.000	−0.000	−0.000	−0.000	0.000	0.000	0.000*	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
FL-Score	−0.022***	−0.020***	−0.001	−0.001	0.002	0.003	0.019***	0.017***	0.002	0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.007)
Risk Preference	0.006	0.007	−0.015*	−0.015	0.018**	0.015*	−0.008	−0.007	−0.001	−0.000
	(0.006)	(0.007)	(0.008)	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
Self-Control	−0.002	−0.002	0.002	0.002	−0.001	−0.000	0.002	0.002	−0.001	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Main Inc. Farming	0.131***	0.136***	0.032	0.035	0.008	0.011	0.058	0.053	−0.230***	−0.236***
	(0.034)	(0.037)	(0.041)	(0.044)	(0.053)	(0.055)	(0.060)	(0.062)	(0.069)	(0.070)
Main Inc. Employed	0.184***	0.197***	0.086*	0.089*	0.046	0.034	−0.021	−0.021	−0.295***	−0.298***
	(0.028)	(0.033)	(0.049)	(0.049)	(0.059)	(0.064)	(0.054)	(0.057)	(0.078)	(0.080)
Main Inc. Self-Emp.	0.144***	0.155***	0.116	0.107	−0.146**	−0.145**	0.070	0.073	−0.184*	−0.190**
	(0.046)	(0.047)	(0.071)	(0.071)	(0.062)	(0.061)	(0.068)	(0.071)	(0.091)	(0.087)
Main Inc. Remitt.	0.075*	0.089**	0.001	0.007	0.103	0.094	0.062	0.060	−0.241***	−0.251***
	(0.036)	(0.040)	(0.043)	(0.040)	(0.067)	(0.071)	(0.066)	(0.067)	(0.083)	(0.083)
Children (0-6 yrs)	−0.006	−0.002	0.045	0.044	−0.011	−0.020	−0.022	−0.019	−0.006	−0.003
	(0.021)	(0.020)	(0.028)	(0.030)	(0.023)	(0.023)	(0.028)	(0.029)	(0.030)	(0.028)
Children (7-10 yrs)	−0.038	−0.038	0.004	−0.009	0.094**	0.095**	−0.039*	−0.035	−0.021	−0.014
	(0.031)	(0.032)	(0.031)	(0.029)	(0.035)	(0.034)	(0.021)	(0.022)	(0.025)	(0.025)
Children (11-16 yrs)	0.028	0.027	0.023	0.018	−0.028	−0.028	−0.000	0.004	−0.023	−0.021
	(0.032)	(0.032)	(0.032)	(0.032)	(0.024)	(0.023)	(0.025)	(0.025)	(0.021)	(0.019)
No. of Elders	0.047**	0.045**	0.026	0.024	0.008	0.008	−0.023	−0.017	−0.058**	−0.060**
	(0.018)	(0.019)	(0.019)	(0.020)	(0.019)	(0.018)	(0.017)	(0.019)	(0.022)	(0.023)
No. of Working Mem.	0.021	0.019	0.037*	0.035*	−0.003	0.000	−0.004	−0.004	−0.050**	−0.050**
	(0.016)	(0.016)	(0.018)	(0.019)	(0.019)	(0.018)	(0.016)	(0.017)	(0.018)	(0.018)
Total HH Education	−0.003*	−0.003*	−0.000	−0.000	−0.003	−0.003	0.001	0.001	0.005**	0.005**
	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Social Status	−0.021	−0.028	−0.015	−0.015	−0.031	−0.034	0.010	0.006	0.057**	0.070**
	(0.018)	(0.018)	(0.027)	(0.028)	(0.023)	(0.025)	(0.021)	(0.021)	(0.023)	(0.025)
Farming Shocks		0.000		−0.000		0.001		−0.000		−0.001**
		(0.001)		(0.001)		(0.001)		(0.001)		(0.000)
Environ. Shocks		0.002		0.001		−0.001		−0.000		−0.002**
		(0.001)		(0.001)		(0.000)		(0.000)		(0.001)
Economic Shocks		−0.000		−0.000		−0.001		0.000		0.001
		(0.000)		(0.001)		(0.001)		(0.001)		(0.001)
Crime Shocks		−0.006**		−0.001		−0.003**		0.000		0.009**
		(0.002)		(0.004)		(0.001)		(0.001)		(0.004)
Other Shocks		0.000*		−0.000		0.000		−0.000		−0.000*
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
Certainty		−0.001		−0.012		0.020		0.033		−0.040*
		(0.022)		(0.024)		(0.034)		(0.022)		(0.019)
Constant	−0.286	−0.363*	0.035	0.151	0.209	0.089	0.207	0.075	0.835***	1.047***
	(0.219)	(0.209)	(0.259)	(0.267)	(0.264)	(0.280)	(0.220)	(0.247)	(0.261)	(0.269)
Observations	676	664	676	664	676	664	676	664	676	664
Adj. R-squared	0.221	0.224	0.025	0.017	0.041	0.037	0.063	0.055	0.072	0.087

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Description of Variables

Debt Indices	
Objective Over-Indebtedness Index	It contains the equally weighted average of z-scores of four debt indicators. The procedure of aggregating these specific outcomes is adapted from Kling et al. (2007). It "improves statistical power" and helps "to detect effects that go in the same direction" among indicators (Kling et al., 2007, p.89). The objective over-indebtedness index captures households with a debt service to income ratio greater than 40%, a remaining debt service to income ratio greater than 40%, households, who defaulted on a loan or paid late in the last 12 months and households with more than two loans. The literature has defined (kind of arbitrary) thresholds for the DSR indicator beyond which a household is over-indebted. A household is deemed over-indebted, for example, if its DSR exceeds - depending on the study - 0.3 to 0.5 (Chichaibelu and Waibel, 2017). Hence, we set the over-indebtedness threshold at a DSR of 0.4 following what we deem is best practice among researchers (Georgarakos et al., 2010).
Subjective Over-indebtedness Index	It contains the equally weighted average of z-scores of three debt indicators: the standardized sacrifice index and two assessments on whether the household has too much debt and whether it has difficulties paying them off.
Debt Measures	
Debt Service to Income Ratio	It is the ratio of all annual interest and principal payments on loans divided by all annual income generating activities of the household.
Debt Position	The question if the household has too much debt right now is asked twice in almost identical fashion. For this reason, we combine both questions by deriving two dummy variables, standardize them and calculate their mean. The exact formulation of both questions is the following: "I have too much debt right now" (Disagree fully, disagree strongly, disagree a little, neither agree nor disagree, agree a little, agree strongly, agree fully) and "Which of the following best describes your current debt position?" (I have too little debt; I have about the right amount of debt; I have too much debt right now.). The first dummy equals 1 if the respondent at least agrees a little and the second equals 1 if they feel they have too much debt right now.
Difficulties to Pay Off Debt	Dummy variable derived from the categorical question with answer options 1-"I have no difficulties paying off my debt", 2-"I have some difficulties [...]", and 3-"I have a lot of difficulties [...]", where 1 and 2 are coded to 0 in the dummy and 3 is coded to 1.

Remaining Debt to Income Ratio	The ratio relates a household's actual, yearly debt burden to the average income of 2016 and 2017.
Sacrifice Index	This index is adapted by , which asks for several sacrifices households may make because they lack money. Like them, we combine these indicators into one "sacrifice index" applying polichoric principal component analysis such that a continuous index is created giving more weight to more serious sacrifices people have to make and transforming the categorical responses into a continuous measure In total, we ask respondents about ten possible sacrifices both for a shorter term (i.e. twelve months) and for a longer term (five years). Unlike , we do not pose questions about the acceptability of sacrifices made but ask only for the frequency of distress events that occurred in the household. We added two questions introduced by and two new questions that are more context-specific to the rural setting in North-East Thailand. Depending on the question asked, respondents could answer on a scale from 1-3 (e.g. had to work much more, more, not more) or from 1-5 (e.g. had to buy less food: never, sometimes, regularly, often, almost always, always).
Income Forecasts	
Quantitative Income Forecast	Relative change between expected median income from the probabilistic expectations elicitation and the actual income in 2017.
Qualitative Forecast Error	Difference between expected income in 2016 and actual welfare of the household as evaluated in 2017.
Expectation Measures	
Actual welfare of the household	Answer to "Do you think your household is better off than last year?", from 1-"much worse off" to 5-"much better off".
Certainty	Answer to "How certain are you that this income development will truly become reality?". The scale ranges from 1-"Very uncertain" to 4 "Very certain".
Expected income	Answer to "How do you think your average monthly income will develop in the next twelve months?", from 1-"Decrease a lot" to 5-"Increase a lot".
Probabilistic expectations	Probabilities assessing how individuals assess future outcomes.

Experiment Measures	
Treatment	1=Hard Quiz, 2=Easy Quiz.
Expected Rank	Rank that participant expects to reach after taking the test quiz from 1-"Least questions answered correctly" to 10-"Most questions answered correctly".
Number of Goods	Amount of goods participant wants to buy.
Overconfidence	Difference between expected and actual rank of participant.
Overborrowing	Dummy variable, that takes the value 1 if participant wants to buy more than earnings including endowment can pay for.
Overspending	Dummy variable, that takes the value 1 if participant wants to buy more than earnings excluding endowment can pay for.
Controls	
Age	Age of respondent in years.
Age Squared	Squared term of age.
Financial Literacy Score	Our index is based on seven questions eliciting financial knowledge, on nine assessments concerning financial behavior, and on three questions regarding financial attitude. The overall index is composed of the sum of the sub indices and ranges between 0 and 22 with higher numbers indicating a higher level of financial literacy.
Financial Risk Taking	Answer to "Attitudes towards risk change in different situations. When thinking about investing and borrowing are you a person who is fully prepared to take risk or do you try and avoid taking risk?", from 1-"Fully unwilling to take risks" to 7-"Fully willing to take risks". Part of our risk preference measure.
Main Income Dummies	We include four income dummies that tell us whether the main income comes from farming, off-farm employment, self employment or remittances.
Monthly Inc. 2017	Monthly household income in 2017
Number of children	This variable is split in three age categories for the analysis. Number of children aged 0-6 years; Number of children aged 7-10 years; Number of children aged 11-16 years.

Number of Elders	Number of elder household members, defined as people older than 60 years.
Shock loss indicators	We include information on monetary losses from various shock events for 2016 and 2017. We hereby separate by five shock categories: Farming Shocks, Environmental Shocks, Economic Shocks, Crime Shocks, Other Shocks.
Number of Working Members	Number of working household members.
Risk Preference	Equally weighted average of risk taking and financial risk taking.
Risk Taking	Answer to "Are you generally a person who is fully prepared to take risks or do you try to avoid taking risk?", from 1-"Fully unwilling to take risks" to 7-"Fully willing to take risks". Part of our risk preference measure.
Self-Control	We use the questions introduced by and add up the Likert-Scale answers to one score. The scale ranges from 1-"Disagree fully" to 7-"Agree fully". The final score ranges from 0 to 49 where lower numbers indicate a higher level of self-control.
Total HH Education	Sum of years all working household members went to school.
Big Five - Personality Traits	
Agreeableness	A person, who scores high on Agreeableness (Item scale ranges from 1 to 7 for all items) has a forgiving nature, is considerate and kind and not rude to others.
Conscientiousness	A person, who scores high on Conscientiousness does a thorough job, works efficiently and is not lazy.
Extraversion	A person, who scores high on Extraversion is communicative, talkative, outgoing and not reserved.
Neuroticism	A person, who scores high on Neuroticism worries a lot, gets nervous easily and is not relaxed.
Openness	A person, who scores high on Openness values artistic experiences, is original and has an active imagination.

Additional Controls Experiment	
BMI	Respondent's Body Mass Index as of 2017.
Difficulties in Game	Answer to "Did the respondent have difficulties answering questions?" with 1-"Not at all", 2-"Yes, a little bit", 3-"Yes, very much". Filled in by the enumerator.
Financial Decision Maker	Answer to question "Who is responsible for making day-to-day decisions about money in your household?" where means 1-"Myself", 2-"Myself and someone else" and 3-"Someone else".
Health Status	Health status of the respondent in 2017: 1-"Good", 2-"Can manage", 3-"Sick"
Marital Status	Respondent's marital status: 1-"Unmarried", 2-"Married", 3-"Widow", 4-"Divorced/separated".
Morning	Dummy variable that takes the value 1 if the interview took place in the morning, i.e. before 11am.
Midday	Dummy variable that takes the value 1 if the interview took place around noon, i.e. between 12am and 2pm.
Numeracy	The numeracy index is based on six questions about simple arithmetic problems. It ranges between zero and six. Zero, if the respondent does not give any correct answer and six if the respondent gives only correct answers.
Read Alone	Dummy variable that takes the value 1 if the participant could read the experimental instructions without help. Filled in by the enumerator.
Relation to HH Head	Respondent's relation to the household head: 1-"Head", 2-"Wife/Husband", 3-"Son/Daughter", 4-"Son/Daughter in law", 5-"Father/Mother", 8-"Grandchild", 9-"Nephew/Niece", 11-"Other relatives".
Sex	Sex of respondent: 1-"Male", 2-"Female".
Years of Schooling	Years respondent went to school.

Experimental Material

Material D.4.1: Instructions Experiment

Experiment Script

Read out:
We want to play a market game with you. In this game you can earn money and buy goods. The kind of goods you can buy are placed right next to you. Each piece has a value of 20 THB, but we offer them to you for a discounted price of 10 THB. You don't have to buy one kind of product, but can buy different kinds (for example 2 chocolate bars and 1 bag of chips). If you don't like to buy anything you can keep the money you earn.

To earn money, you have to play a quiz which consists of 15 questions. 10 persons from another village, which is similar to your village, took the same quiz before. The amount of money you earn is dependent on how many questions you answered right in comparison to these villagers. In this picture, the person who has given the most correct answers is ranked 10, the person who has given the second most correct answers is ranked 9, the person who has given the third most correct answers is ranked 8, and so on. In the picture you can also see how much money you will earn dependent on your ranking. For example, if you are ranked 7 you will earn 20 THB. Please take your time to understand how you can earn money in this game.

[Show picture of ranks, payoffs and people]

I want you to ask some test question to check whether the procedure of the ranking is clear to you. If not, I will explain it again.

Test Question 1: What does it mean to be ranked 6? [Open answer; enumerator please continue if you think the respondent gave a correct answer]

Test Question 2: How much money do you earn if you are ranked 6? [Answer: 10 THB]

Test Question 3: How many goods you can buy for 10 THB? [Answer: 1]

The money you earn, will be put on your game account which already has 40 THB in it. As you can see from the picture, you can earn up to additional 40 THB. The quiz for which you will receive money will be played in the second round.

In the first round, you will get 7 test questions, which are very similar to the questions you will get in the second round. But again, you can ONLY earn money in the second round.

After you answered this first set of questions, you have to decide how many goods you want to buy. The 40 THB that are already in your account are given you as a credit that you can use to buy the goods. With the money you earn in the second round in the quiz you will pay back your credit. If you spend more money than you earned we will keep the money from your account and give you the goods you have bought. If you earned more than you bought, you pay back your credit and can keep the rest of the money and goods.

If you don't have any further questions we start with the first round. [FAQ]

[Hand respondent the first quiz (green paper). If respondent cannot read, assist in all tasks]

Please read through the questions on the green sheet of paper and try to answer as many questions as you can. You have 5 minutes to answer the questions. I will tell you when the 5 minutes are over. After you have finished the quiz, please have a look on the white piece of paper and answer these questions and make your buying decision. When you have finished the first round, I will collect the white piece of paper. You can keep the green paper with the test quiz. It is only for you, so that you know what kind of questions to expect in the quiz of the second round.

[Set your alarm clock to 5 minutes and tell the respondent to start]

The 5 minutes are over. Please, stop answering the test quiz and make your decisions on the white sheet of paper. Give me a sign when you have made your decisions, then I will collect the white paper.

[During the time the respondent takes the second quiz, evaluate the white sheet of paper and enter the numbers on the tablet]

Now, in the second round, you play the quiz that decides how much money you earn. You have 10 minutes to answer the questions. Afterwards, I will collect the quiz, calculate your earnings and hand you the goods and money.

[Hand the second quiz, set your alarm clock to 10 minutes and tell respondent to start]

The time is up. Please, hand me the second quiz. Before we conclude, I have some final questions for you.

Question 1: After taking the quiz, when 1 is the villager who gave the least correct answers and 10 is the villager who gave the most correct answers, where do you see yourself in this picture?

Question 2 [Only ask if expected earning of respondent was smaller than 40 THB]: Would you have buy more goods, if you thought your earnings would be higher?

Question 3 [Only ask if expected earning of respondent was more than 0 THB]: Would you have buy less goods, if you thought your earnings would be lower?

Thank you very much for your participation, we hope you enjoyed the game. I will now calculate your earnings and inform my STL which will bring you your payment and goods.

[Calculate rank, earnings and cash/goods payoff. Wait for STL to hand the money/goods]

{In the very unlikely case, that more goods were wanted than earnings are generated:}

I calculated your earnings and you cannot afford all the goods you want to buy. You want to buy […] goods but can only afford […] goods. Please, choose which goods you want to keep.

[Please note which goods were finally kept]

Material D.4.2: Guideline for Interviewers to Answer Questions from Participants

Frequently Asked Questions

Respondent: "What if I don't want to buy anything?"
You: "You don't have to buy anything, you can also keep the money."

Respondent: "Can I spend all my money on buying products?"
You: "Yes you can, but if you do not earn enough money to pay all the products you wanted to buy, you will only get the part of the products you can afford."

Respondent: "Can I change my buying decision after I took the second quiz?"
You: "No, your decision is fixed. Only in the case where you wanted to buy more products than you have money available, you can decide on which products to keep"

Respondent: "What happens if I spend more money on products than I earn?"
You: "Then we will take the money from the 40 THB that are already on your virtual bank account for the game. If even this is not enough, you only get as many products as you have money. We will NOT take any out of your pocket and we will NOT take money from the 50 THB you get for the questionnaire. We only count the money you get in the game."

Respondent: "Does being on rank 7 means that I need to get 7 questions correct?"
You: "No! It means that three persons have answered more questions correctly than you and six persons have answered less questions correctly than you. The rank is always dependent on how many questions you have correct in comparison to the other 10 villagers. In this case you are as good as the villager who was ranked 7."

Respondent: "Does it make a difference which questions I answer correctly?"
You: "No, all questions count the same."

Respondent: "Do the products really cost 20 THB per piece?"
You: "Yes, if you buy them as presented here, they cost 20 THB."

[Respondent: "What if I don't know the answer to a question at all?"
You: "Just take a guess. You don't receive some sort of minus points for wrong answers."]

Respondent: "What if I cannot finish the quiz in time?"
You: "That is no problem. Please, try to answer as many questions as you can in the given time frame. There will be no minus points for unanswered questions."

Respondent: "Who are the other 10 persons who have answered the quiz before?"
You: "They are just some randomly selected persons from another village, that is similar to your village."

Material D.4.3: Quiz-Hard Treatment

Test Quiz

1. What is the biggest city in Canada by population?

- ☐ Ottawa
- ☐ Vancouver
- ☐ Montreal
- ☐ Toronto

2. What is the most common blood type in the world?

- ☐ O positive
- ☐ AB positive
- ☐ B positive
- ☐ A positive

3. Which animal cannot fly?

- ☐ Chicken
- ☐ Duck
- ☐ Penguin
- ☐ Squirrel

4. Which fruit contains the most amount of Vitamin C per 100g?

- ☐ Pineapple
- ☐ Mango
- ☐ Banana
- ☐ Passion Fruit

5. How many days does Mercury need to orbit the sun?

- ☐ 144
- ☐ 94
- ☐ 88
- ☐ 126

6. Which animal is not part of the Zodiac?

- ☐ Leo
- ☐ Pisces
- ☐ Dragon
- ☐ Scorpio

7. Which are the Japanese cities that were hit by atomic bombs of the U.S. army during WWII?

- ☐ Hokkaido and Kyushu
- ☐ Shikoku and Hashima
- ☐ Okinawa and Okinoshima
- ☐ Hiroshima and Nagasaki

Quiz

1. What is the national animal of China?

- [ ] Tiger
- [ ] Eagle
- [ ] Lion
- [ ] Panda

2. If Thai currency is THB, what is the currency of Germany?

- [ ] Euro
- [ ] US. Dollar
- [ ] Pound
- [ ] Deutsche Mark

3. How many provinces does Japan have currently?

- [ ] 47 provinces
- [ ] 48 provinces
- [ ] 49 provinces
- [ ] 50 provinces

4. Which is the heaviest insect in the world?

- [ ] Grasshopper
- [ ] Spider
- [ ] Beetle
- [ ] Centipede

5. Which of these countries does NOT border Germany?

- [ ] Austria
- [ ] France
- [ ] Sweden
- [ ] Poland

6. Which is the most drank beverage in the world?

- [ ] Coca Cola
- [ ] Beer
- [ ] Tea
- [ ] Coffee

7. Which country is the origin of pizza?

- [ ] Italy
- [ ] France
- [ ] Spain
- [ ] Portugal

8. Which of these four is the biggest organ of the human body?

- [ ] Lungs
- [ ] Heart
- [ ] Liver
- [ ] Brain

9. Who is the president of Indonesia?

- ☐ Susilo Bambang Yudhoyono
- ☐ Joko Widodo
- ☐ Abdurrahman Wahid
- ☐ Megawati Sukarnoputri

10. What color is traditionally not associated with Christmas Day?

- ☐ Red
- ☐ Gold
- ☐ Green
- ☐ Pink

11. Of which colors is the flag of Germany composed of?

- ☐ Black, Blue and Gold
- ☐ Black, Red and White
- ☐ Black, Red and Gold
- ☐ Black, Red and Blue

12. Which fruit is blue?

- ☐ Blueberry
- ☐ Pear
- ☐ Apple
- ☐ Kiwi

13. What color will you get if you mix blue, red and yellow?

- ☐ Grey
- ☐ Dark green
- ☐ Black
- ☐ Brown

14. How many seasons are there in Germany? And which ones?

- ☐ 4 seasons including spring, summer, autumn and winter.
- ☐ 3 seasons including rainy, winter and spring
- ☐ 2 seasons including rainy and winter
- ☐ 2 seasons including summer and winter

15. Who is the God of Islam?

- ☐ Nabi Muhammad
- ☐ Yahweh
- ☐ Allah
- ☐ Moses

QID: ☐

Material D.4.4: Quiz-Easy Treatment

Test Quiz

1. What is the biggest city in Thailand?

- ☐ Ubon Ratchathani
- ☐ Chiang Mai
- ☐ Bangkok
- ☐ Surat Thani

2. What color will you get if you mix blue and yellow?

- ☐ Grey
- ☐ Green
- ☐ White
- ☐ Pink

3. Which animal cannot jump?

- ☐ Asian Buffalo
- ☐ Dog
- ☐ Elephant
- ☐ Tiger

4. Which fruit is prohibited in public transport around South-East Asia?

- ☐ Banana
- ☐ Papaya
- ☐ Durian
- ☐ Apple

5. Which of these countries does NOT border Thailand?

- ☐ Vietnam
- ☐ Laos
- ☐ Cambodia
- ☐ Myanmar

6. Which animal is not part of the Chinese Zodiac?

- ☐ Monkey
- ☐ Horse
- ☐ Cat
- ☐ Dragon

7. What is the most common eye color in the world?

- ☐ Blue
- ☐ Brown
- ☐ Green
- ☐ Hazel

Quiz

1. What is the national animal of Thailand?

- [ ] Elephant
- [ ] Eagle
- [ ] Lion
- [ ] Naga (Thai Dragon)

2. If Thai currency is THB, what is the currency of USA?

- [ ] Euro
- [ ] US Dollar
- [ ] Pound
- [ ] Franc

3. How many provinces does Thailand have currently?

- [ ] 76 provinces
- [ ] 77 provinces
- [ ] 78 provinces
- [ ] 79 provinces

4. Which is the biggest animal in the world?

- [ ] Blue Shark
- [ ] Killer Whale
- [ ] Blue Whale
- [ ] Elephant

5. How many months have 31 days?

- [ ] 6
- [ ] 5
- [ ] 4
- [ ] 7

6. How many seasons are there in Thailand? And which ones?

- [ ] 3 seasons including summer, rainy and winter
- [ ] 2 seasons including summer and rainy
- [ ] 2 seasons including rainy and winter
- [ ] 4 seasons including summer, rainy, autumn and winter

7. Which of these do you need to make traditional Som Tam Thai?

- [ ] Coconut Milk
- [ ] Tomatoes
- [ ] Oyster Sauce
- [ ] Chili Paste

8. Which is the biggest sense organ of the human body?

- [ ] Skin
- [ ] Eyes
- [ ] Mouth
- [ ] Ears

9. Who is currently the president of the United States of America?

- [ ] Donald Trump
- [ ] Barack Obama
- [ ] Angela Merkel
- [ ] Bill Clinton

10. What is the color of the day on Wednesday?

- [ ] Red
- [ ] Pink
- [ ] Green
- [ ] Light blue

11. Of which colors is the flag of Thailand composed of?

- [ ] Green, White and Red
- [ ] Green, White and Blue
- [ ] Blue, White and Red
- [ ] Blue, Red and Yellow

12. Which fruit does not have thorns?

- [ ] Durian
- [ ] Jackfruit
- [ ] Rambutan
- [ ] Salak

13. Which reign of Thailand abolished slavery?

- [ ] 4th Reign
- [ ] 5th Reign
- [ ] 6th Reign
- [ ] 7th Reign

14. Which country has the highest total rice consumption?

- [ ] Thailan
- [ ] Germany
- [ ] Cambodia
- [ ] China

15. Who is the son of god of Christianity?

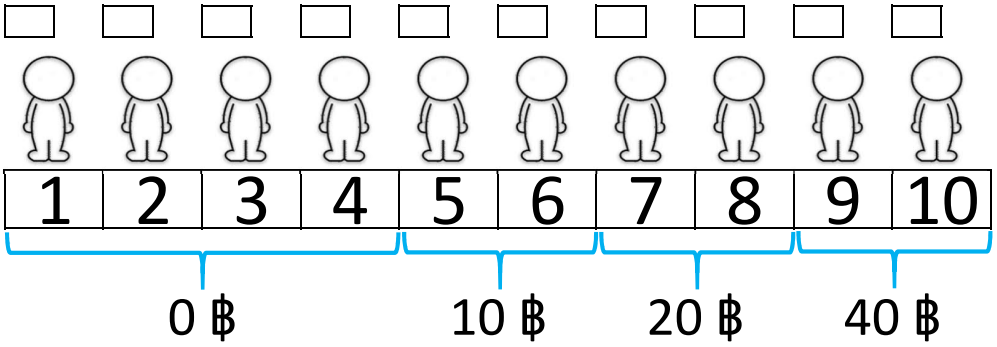
- [ ] Nabi Muhammad
- [ ] Jesus
- [ ] Guanyin
- [ ] Vishu

QID: [ ]

Material D.4.5: Decision Sheet

Before you take the second quiz where you can earn money, we have some questions for you and you have to decide which goods and how many you want to buy.

Question 1: As mentioned before, 10 persons from another village took the same quiz as you will have to take now. After taking the test quiz and knowing the second quiz will be similar: When the villager on the left side of this picture is the one who gave the least correct answers and the villager on the right side of this picture is the one who gave the most correct answers, where do you see yourself in this picture? Please cross the respective box.



Question 2: We told you that the money you will earn in the second quiz depends on how you actually are ranked in this picture above. For example if you are ranked 7, which means that 3 villagers gave more correct answers than you and 6 villagers gave less correct answers than you, you will get 20 THB. What do you think, how much money will you earn?

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Question 3: Now, you have to decide how many and which kind of goods you want. You have to think about how much you will possibly earn including your credit and how much you can spend on the goods. You don't have to buy anything at all. But if you want to, remember each piece has a discounted price of 10 THB and you can buy as many different kinds as you want.

Example: You think you are ranked 7, so you earn 40 THB, and you want to buy one pack of coffee and one bag of chips. That will cost you 20 THB. After you have answered the second quiz, we will calculate your earnings.

If you have earned 40 THB for example, we will give you the goods you wanted to buy and additionally 20 THB.
All in all, you have two goods then and 60 THB.

If you have earned 10 THB for example, we will give you the goods you wanted to buy and we will deduct 10 THB from the 40 THB credit we gave you. All in all, you have two goods then and 30 THB.

Please indicate here how many of each good you want. If you do not want to buy some kind of good put 0 there:

Coffee		Mango	
Chips		Detergent	

QID:

Erklärung

Zur Erstellung dieser Dissertation habe ich folgende Hilfsmittel genutzt: Google Forms, hroot, Latex, MATLAB, MS Word, MS Excel, ORSEE, oTree, Stata, Survey Solutions, TeXstudio sowie z-Tree. Des Weiteren habe ich neben eigen erhobenen Daten, Daten des Thailand Vietnam Socio Economic Panels verwendet.

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, den 06. Januar 2020

Melanie Koch